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GILBERT AND THE HISTORIANS (I) *

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1 *Three Views of Gilbert's Science*

GILBERT's *De Magnete*, first published in 1600, appeared at a strategic point in the history of science, when the Renaissance era overlapped the beginnings of the scientific revolution, and its importance for an understanding of the origins of that revolution is obvious. In spite, or perhaps because, of this importance, the work has been subject to more contradictory reports and assessments than almost any other classic in the history of science.[1] Broadly speaking the views of the historians on Gilberts' scientific method fall into three classes. First there are those who, in commending his painstaking and exhaustive experimental studies, claim him as the first Baconian scientist, practising Bacon's method twenty years before the publication of the *Novum Organum*. But then it has to be admitted that there are passages in the *De Magnete*, and even more in Gilbert's posthumous work *Philosophia Nova*,[2] which do not fit into any simple picture of Baconian science, and so a second Gilbert takes shape: a metaphysician, Neo-Platonist, scholastic, animist,

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* Received 5. vi. 59.

[1] See, for example, W. Whewell, *History of the Inductive Sciences*, 1837, **3**, 45; and *Philosophy of the Inductive Sciences*, 1840, **2**, 374; Park Benjamin, *Intellectual Rise in Electricity*, 1895, pp. 258-331; A. Wolf, *A History of Science, Technology and Philosophy in the XVIth and XVIIth Centuries*; A. Koyré, *Etudes Galiléennes*, 1939, pp. 100, 148, 249; and *From the Closed World to the Infinite Universe*, 1957, p. 131; G. Sarton, *Six Wings*, 1957, p. 94; G. K. Chalmers, ' The Lodestone and the Understanding of Matter in Seventeenth Century England', *Philosophy of Science*, 1937, **4**, 75; E. Zilsel, ' The Origins of William Gilbert's Scientific Method ', *Journal of the History of Ideas*, 1941, **2**, 1; M. Boas, ' Bacon and Gilbert ', *J.H.I.* 1951, **12**, 466; C. Boyer, ' William Gilbert on the Rainbow ', *Am. Journ. Phys.* 1952, **20**, 416; D. H. D. Roller, ' Did Bacon know Gilbert's *De Magnete?* ', *Isis*, 1953, **44**, 10; J. Agassi, ' Koyré on the History of Cosmology ', this *Journal*, 1958, **9**, 234.

[2] Gilbert, *De Mundo Nostro Sublunari Philosophia Nova*, Amsterdam, 1651

or fuzzy speculator; 'a Renaissance figure, balanced between the modern spirit of experimentation and the medieval spirit of speculation'.[1] The apparent contradiction between these two Gilberts is commonly resolved with the remark that, fortunately, the deposit of enduring fact in his work can be separated from the ephemeral speculations, so that, to paraphrase Bacon, there remains at the bottom, all light opinions vanishing into smoke, a Fact affirmative, solid and true and well defined.

A third view is possible, however, in which the various aspects of Gilbert's science appear more integrated and intelligible. The recent reissue of the Thompson translation of *De Magnete*,[2] and the appearance of Popper's *Logic of Scientific Discovery* in an English edition, together provide an opportunity for reassessment of Gilbert's work in the light of a different analysis of the relation between fact and theory from that presupposed by most of his historical commentators. It is not yet possible to see in detail what effect Popper's thesis will have on commonly accepted interpretations in the history of science, but it is certain that, if taken seriously, its effect will be far-reaching, for most history of science has hitherto been written from the point of view of what Popper attacks under the name of *inductivism.* By this he refers to the beliefs that (1) there are basic descriptions of fact which can be known to be true by experience, and (2) empirical science is derived from these by means of inductive methods of generalisation and theory-construction. Part of Popper's reply[3] consists in the counter-assertions that (1) every descriptive statement is itself open to question since it will always go far beyond what can be known with certainty on the basis of experience, and (2) the criterion which marks off empirical from metaphysical theories is not that empirical theories are inductively *based on* empirical facts, but that they are *falsifiable* by statements about conceivable empirical facts. There is no need to expand Popper's argument here, for it is well known, and what I propose to do in this paper is to illustrate these two points by considering Gilbert's work, and the historical interpretations of it I have mentioned, and hence to show that the third view of Gilbert which Popper's thesis suggests is more satisfactory than the traditional interpretations.

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[1] M. Boas, op. cit., p. 467

[2] Gilbert, *On the Magnet, Magnetic Bodies also, and on the great magnet the earth; a new Physiology, demonstrated by many arguments and experiments*, trans. S. P. Thompson, ed. D. J. Price, Basic Books, Inc., New York, 1958. This translation was first published in 1900, and had been preceded in 1893 by a translation by P. Fleury Mottelay. Page references in brackets in the text will be to the 1958 edition. I have occasionally modified the translation in quotations.

[3] K. R. Popper, *Logic of Scientific Discovery*, London, 1959, pp. 40 f, 78 f, 93 f

If Popper's two points are accepted as against the inductivist view, then it will follow that the inductivist historians have made their division into the two Gilberts in the wrong place. For the inductivist, the distinction will be between Gilbert the experimenter who faithfully reports what he observes and whose reports are therefore infallible and lasting, and Gilbert the speculator, who carelessly allows his theoretical preconceptions to colour his experimental reports, who illegitimately generalises to conclusions not warranted by the facts, and whose theories have therefore turned out to be false and useless and fit to be consigned, as Gilbert himself consigned those of his predecessors, 'to the moths and worms' (64). For Popper, on the other hand, the distinction will be between Gilbert the empirical scientist, who states generalisations and theories and then subjects them to stringent tests and whose experimental reports themselves *always* go beyond sense-experience and are therefore subject to further test, and Gilbert the metaphysician who suggests theories which he does not subject to tests which may lead to falsification, or which are in principle unfalsifiable. Our judgment of Gilbert's standing as an empirical scientist will clearly be modified by an acceptance of the second of these distinctions rather than the first, and a further modification will take place if we admit (as Popper himself asserts[1]) that metaphysics is not wholly irrelevant to the logic of scientific discovery.

I shall try first to deal with the inductivist claim that most of Gilbert's experiments are modern in spirit and permanently acceptable *because* he did not allow theoretical conceptions to interfere with them, and secondly I shall try to assess Gilbert's theories in the light of the criterion of falsifiability.

2 *Gilbert's 'new sort of Philosophising'*

> To you alone, true philosophizers, honest men, who seek knowledge not from books only but from things themselves, have I addressed these magnetical principles in this new sort of Philosophizing. . . . Whoso desireth to make trial of the same experiments, let him handle the substances, not negligently and carelessly, but prudently, deftly, and

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[1] See for example K. R. Popper, 'The Nature of Philosophical Problems and their Roots in Science', this *Journal*, 1952, 3, 124.

> in the proper way . . . for nothing hath been set down in these books which hath not been explored and many times performed and repeated amongst us [Preface to the Reader].

Thus Gilbert professes his adherence to the principle of the repeatable experiment as the foundation of natural philosophy. Widespread acceptance of this principle was undoubtedly one of the new features of the scientific revolution, and its importance is not in dispute among philosophers of science. This is not the place to discuss its historical origins, but it is interesting to remark on the self-conscious manner in which it was analysed by Gilbert and his contemporaries. For them the thesis that detailed and even tedious experiment is profitable and necessary is not a platitude but a platform, requiring all the rhetorical and polemical arts. For Bacon it is the gospel which brings salvation from the natural sins of the intellect; for Galileo, speaking of Gilbert, adherence to it requires god-like qualities:

> Since we see that the ordinary human mind has so little curiosity and cares so little for rare and gentle things that no desire to learn is stirred within it by seeing and hearing these practised exquisitely by experts . . . these [Gilbert's inventions] are concepts and ideas for superhuman souls.[1]

The dispute between inductivists and others does not arise at the level of the decision to rely on repeatable experiments, but at a question which is logically posterior to this, namely the question of the status of verbal reports of such experiments. The inductivist holds that such reports should, if made with proper care and open-mindedness, be statements of fact which will remain invariant to all subsequent changes in the theories designed to explain the facts. This is the presupposition of the account of Gilbert's work which distinguishes the true deposit of experiment from the froth of theoretical misinterpretation and speculation. Let us therefore look more closely at some of his experiments.

In some cases it is clear that the design of the experiments is determined by Gilbert's cosmology. There is no doubt that he himself and his immediate sucessors considered his major achievement to have been the demonstration that the earth is a 'great magnet'. This is apparent in the title of his book and throughout its text, as well as in the comments upon it of (among others) Kepler, Galileo, Bacon, Descartes, and Boyle. None of these positively accept Gilbert's wider cosmological speculations, and some reject them, but all except Bacon regard the theory of the magnetic nature of the earth as proved.[1] Gilbert's ' proof ' consists of experiments on a model which reproduces the earth's form as far as possible, that is, on a spherical lodestone, or as he calls it a ' terrella '. That he should conduct most of his researches into magnetism with lodestones of this shape is therefore determined by his theory that the magnetic power and shape of the earth are closely connected, and that there is in this particular composite form—spherical magnetic substance—something elementary and fundamental in the structure of nature. The earth is the active and powerful mother in whose womb, as it were, all secondary materials grow (19), it is iron and not gold which is the most noble metal (24), iron and lodestone (which Gilbert considers to have the same form) are primary bodies, and the sphere is the most perfect form (12).

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[1] Galileo, *Dialogue concerning the Two Chief World Systems*, trans. S. Drake, Berkeley & Los Angeles, 1953, p. 406

But, inductivists will argue, here is a clear case where the metaphysical froth can be blown away, leaving some undisputed facts. It was no doubt unfortunate that Gilbert chose to experiment with a magnet whose shape lessened its potential power, but still he discovered how magnetic bodies behave, and when he contented himself with bare descriptions of what he saw, his results stand. In replying to this thesis I shall try to show that if it is true, then it is never possible for the experimenter himself to know which of his reports are in this sense ' bare descriptions ' and which are theory-loaded, and that what counts for the historian as a ' bare description ' is dependent on subsequent theories and therefore relative to the time at which the history is written.[2] Thus the thesis contradicts itself, because one of its premisses is that ' bare descriptions ' are invariant to change of theory. Furthermore these consequences show that it is ridiculous to base any assessments of scientific ability or personal character merely upon the fact that an experimenter reports his experiments in terms of archaic theories.

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[1] Galileo has no doubt that this is a hidden but inferred fact: the superficial appearance of the earth *conceals* a lodestone, but Gilbert's reasons ' conclusively prove *de facto* that our globe is made of lodestone '. And (Sagredo is made to say) ' I rank these experiments with the needle very little lower than mathematical proof '. (Galileo, op. cit., pp. 403, 408.)

[2] An ironical example of how hindsight can pervert the historian's judgment is provided by Bacon's theory of heat. Most modern historians of science have little good to say of Bacon, but they have to admit that the one positive result of his method which is contained in his works, namely that heat is a mode of motion of matter, is ' correct '. Whewell, however, writing at a time when the hypothesis ' that heat consists in the vibrations of an imponderable fluid ' at least included the established laws, while Bacon's included none of them, calls Bacon's hypothesis ' an example of illicit generalisation ' (Whewell, *Philosophy of the Inductive Sciences*, vol. ii, p. 402). It is a measure of Whewell's sense of historical values that this does not prevent him giving a fair and just assessment of Bacon's method and achievements.

We have to show then, that what counts, for the inductivist, as a ' bare description ', is always dependent upon later theories, and this will be done by showing that of various experimental reports which Gilbert would undoubtedly have regarded as careful descriptions, some turn out to be acceptable in the inductivist's sense, and some turn out in this sense to be distorted by discarded theories, and that the way in which these two classes of reports are now distinguished is dependent upon later theories, none of which could have been foreseen by Gilbert. We shall consider Gilbert's distinction between *coition* and *attraction*, his statements about *repulsion*, and his distinction between *rotation* and *coition*.

3 *Experiment and Interpretation*

The only chapter in which Gilbert discusses electric attraction is entitled ' On the Magnetic Coition, and first on the attraction of amber, or more truly, on the attaching of bodies to amber ' (46). In this and the following chapter ' Opinions of others on magnetic coition, which they call attraction ' (60), Gilbert carefully distinguishes between the force of *attraction* or ' allure ' exerted by electrics on light bodies which are drawn to them passively, and magnetic *coition* in which bodies, whether iron or lodestone, ' run together with mutual forces '. In speaking of magnets

> we say coition, not attraction. The word attraction unfortunately crept into magnetic philosophy from the ignorance of the ancients; for there seems to be force applied where there is attraction and an imperious violence dominates. But wherever we have spoken of magnetic attraction, we understand thereby magnetic coition, or a primary running together (60).

Gilbert does not overlook the fact that if a heavy magnet is in the vicinity of a light one, the latter is moved more easily and appears to be ' allured ' by the former (71), and yet he describes similar phenomena in the case of a large piece of amber and light bodies in terms which appear to him to indicate a fundamental difference between the two cases. It is easy to give an explanation of his distinction in terms of his own theoretical background and interests: there was at the time no conception of equality of action and reaction, and the question whether attractions were to be described in terms of one active, externally attracting body drawing another, passive, body; or of an active internal tendency of the attracted body towards the other; or of mutual attractions; was a controversial one. Furthermore, as Park Benjamin points out,[1] it was important for Gilbert to distinguish the causes of electric attraction from those of magnetism as far as possible, because he wished to maintain that lodestone and iron and their properties are alone fundamental and predominant powers in the universe. To see how Gilbert's distinction between attraction and coition fits into this pattern is not however to show that he could have known the distinction to be mistaken. No doubt he conceived himself to be drawing a careful and accurate distinction between two kinds of phenomenon which he observed to be different. And why do we regard the distinction as 'mistaken' and the two cases as identical in respect of the reciprocal nature of the attractions? Only because of our belief in the equality of action and reaction (not stated clearly until Newton), and because of much later theories about charge-induction and dielectrics.

Again, take the question of repulsion. Gilbert states positively that electrics do not repel (55, 113), although he describes experiments in which bodies move more quickly as they approach the electric and in which it is difficult to believe that he did not also observe them bounce off with considerable force, as is later reported by Cabaeus (in 1629) and by Browne (in 1646). Whatever Gilbert observed, he did not report it as repulsion, even though the possibility of repulsion would have accorded better with his theory of effluvia emanating from the electric (55). Even in the case of magnets he reports that magnetic substances are more sluggishly repelled than they are attracted (100), and describes the behaviour of two floating magnets placed with like poles together as not so much a mutual repulsion as a disposition to come into harmony:

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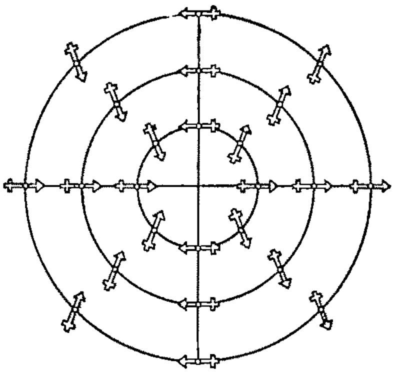
[1] Benjamin, op. cit., pp. 294 f. Although some of Benjamin's comments in his account of Gilbert are typical of what I have called inductivist history, it would be unjust not to remark here that the account is in other respects an unusually perceptive one, and that Benjamin has uncovered two significant points which have helped my own argument against inductivism. The first has just been referred to; the second is Gilbert's interpretation of repulsion (Ibid., pp. 285 f).

> For stone disposeth stone; the one turns the other around, reduces it to range, and brings it back to harmony with itself. When, however, they come together and are conjoined according to the order of nature, they cohere together firmly (16).

Therefore ' there is properly no such thing as magnetic antipathy ' (68). It may be that Gilbert dislikes the notion of a ' repulsion ' just as he dislikes ' attraction ' because it savours of the occult sympathies and antipathies postulated unnecessarily in all kinds of processes which he has himself investigated (109). But there is no reason to doubt that Gilbert is here giving a careful report of what he observes. That we should report the same phenomena in terms of repulsion is due to familiarity with a wider range of theories which lead us to treat attraction and repulsion as symmetrical.



Gilbert's experiment with a terrella and iron spikes (206).

On the other hand an example may be given of an experimental report which does turn out to accord with later theories, but where Gilbert's immediate interpretation of his report does not, and where the experiments are so difficult that the ' correctness ' of the report must be said to be accidental and not anticipatory. The example concerns the cause of the earth's property of directing magnetic needles horizontally towards the north and in the vertical plane of dip. -This property had previously been ascribed to the attraction of the earth for the passive needle, but Gilbert, following the English writer Robert Norman, whose *Newe Attractive* appeared in 1576, denies that the directive property is due to any attraction or coition of the poles, asserting that it is an independent disposing influence of the whole earth. Gilbert quotes two kinds of experiments in support of this conclusion. First he has himself performed experiments with a terrella and iron spikes, in which the spikes cling on to the surface of the terrella, or if suspended above it remain at a definite angle with no tendency to be drawn directly towards the pole of the terrella. And the spikes remain at the same angle whatever the strength of the lodestone, instead of inclining more towards the pole of a stronger lodestone as would be expected if the effect were one of coition (82, 131, 195). The results of these experiments are definite, but they are not carried out in relation to the earth, and in any case the conclusion to be drawn from them is not quite unambiguous, so Gilbert refers to two experiments of a different kind which are designed to show, in the case of the earth, that when the effect of gravity is neutralised, there is no tendency of a magnetic needle to move as a whole towards the earth's poles. Both experiments are quoted from Robert Norman. Gilbert acknowledges that the first is Norman's and does not claim, by his device of marking with asterisk, that he has himself performed it and clearly demonstrated the effect reported. In the first experiment Norman magnetises an iron wire and floats it on the surface of water. It shows, he says, no tendency to move bodily, but only to rotate to point towards the north. Gilbert comments: ' This assertion of the Englishman, Robert Norman, is plausible and appears to do away with attraction ' (162). In the other experiment, Norman arranges that a needle stuck through a cork floats wholly immersed in a glass bowl of water both before and after magnetisation. Again it is not found that the needle moves downwards towards the earth's pole, but only that it rotates about the cork into a dipped position. ' But ', says Gilbert, ' it must be understood that as it is a curious and difficult experiment, so it does not remain long in the middle of the water but sinks at length to the bottom, when the cork has imbibed too much moisture ' (204).

It must be concluded that Gilbert had tried both these experiments and failed to obtain clear evidence of the effect reported by Norman, and yet he is prepared on the basis of them to distinguish coition from the rotating property. The distinction cannot be said to be, for Gilbert, an experimentally based descriptive statement such as an inductivist would approve. And yet, as it happens, Gilbert was right in the light of the later theory according to which the earth exerts a couple but no force on a small magnet in its field. Gilbert himself however cannot approach an explanation in terms of resultant force and couple, since he lacks the notion of repulsion, and his own interpretation of the distinction between coition and rotation does not accord with the later theory. He suggests that the two forces which cause coition and disposition to rotate have different ranges, being spread over, respectively, the 'orb of coition', which is 'all that space through which the smallest magnetic is moved by the lodestone', and the 'orb of virtue', which is 'all that space through which the virtue of any lodestone extends' (*vj).

I have selected three of Gilbert's interpretations of experiments as samples of the way in which description and interpretation are mingled, and only distinguishable in the light of subsequent theories. And this happens when, as in these cases, the experimenter is careful in his methods and precise and detailed in his reports. It may appear at first sight that inductivism is refuted by such examples, since they show that even descriptive reports of experiments are dependent on theories which may later be abandoned—they show in fact that the reports of these experiments were not 'bare' descriptions. It is never possible, however, to *refute* inductivism in this way, or by any other historical examples, because the inductivist may explain away any situation that can arise. If the theory on which an experimental report depends is still accepted, the inductivist can interpret the report as a bare description, and if it is not accepted, he can allege that no true report was given because the experimenter was blinded by his false theory and careless in his observations. Thus an argument against inductivism based upon such examples depends on the historian's judgment of the psychology of the experimenter; that is to say, it has to show that according to the historical evidence the experimenter was *not* blinded or careless, and it is never possible to show this conclusively.

Such arguments may, however, show that inductivism is unplausible, and that there is another, more plausible, account of the historical facts which is not inductivist. This account would speak, not of Gilbert's 'facts' as distinct from his 'theories', but of those of his theories or interpretations which are now accepted in the light of further experience and those that have been refuted by it. But this latter distinction is not of great *historical* interest; it is not a distinction on which to base judgments of the scientific standing of the experimenter, for these must depend on the whole body of theory and experiment available to him in his historical situation and the use he makes of it.

Whipple Museum, Cambridge.

(*To be concluded*)

THE DOPPLER EFFECT AND THE FOUNDATIONS OF PHYSICS (I) *

HERBERT DINGLE

1 *Introduction*

IT is doubtful if there is a serious rival to the Doppler effect as the department of modern science in which the experimental basis is slightest in comparison with the structure raised on it. Most of our knowledge of stellar motions, including the characteristics of binary stars, the evidence for the rotation of the galaxies, and the whole phenomenon known as 'the expansion of the universe' consists of deductions from this one principle. It would be fair to say that without it cosmology would scarcely exist as a scientific subject. Yet the number of cases in which we have independent evidence of a spectrum shift and an associated motion are very few, in all of them the velocities concerned are so small that they will fit a variety of formulae, and in no case at all have we evidence of the reciprocity of the effect—i.e. evidence that if A observes B's spectrum to be displaced, B will observe A's spectrum to be similarly displaced. The original justification for supposing the effect to exist was purely theoretical, and the theory concerned is no longer acceptable. This situation calls for a specially close examination of the various possibilities between which an experimental decision cannot yet be made, and so is an eminently suitable subject for the philosophy of science. It will be seen, I think, that there are problems involved in the understanding of the Doppler effect that are not generally realised, and that take us to the roots of physical theory.

2 *Discovery and Character of the Doppler Effect*

The Doppler effect is the name given to the phenomenon by which the frequency of a source of vibration appears to be increased when the source and the observer are approaching one another, and decreased

* Received 20 v. 59

when they are receding from one another, as compared with the frequency of the same source when it is at rest with respect to the observer. In this article I am considering only the case in which the 'source of vibration' is a source of monochromatic light. The Doppler effect for sound is a far simpler, and purely physical, matter, which arouses no philosophical problems. We can observe directly that the source is vibrating and that the result is a wave motion in the material medium between the source and the observer. We can measure independently the frequency of the source and that of the waves, and verify that they are equal. We can measure independently the velocity V, the frequency n, and the wave-length l of the sound waves, and show that the equation, $V = nl$, is true. No phenomenon is known that is not perfectly conformable to these conceptions.

With light, and the source of light, on the other hand, our knowledge is almost entirely inferential. We can observe no vibration in the source, and we cannot observe light itself at all in any sense; we can make observations only on matter with which we suppose it to interact. According to current theory, the frequency which we assign to the light is not at all identical with that which we assign (partly metaphorically) to the source. The velocity of light we can measure satisfactorily only as an average velocity over a to-and-fro journey, though, with the assistance of mechanical theory, we can get a rough value for a one-way velocity from observations of Jupiter's satellites. Of the nature of light we have the most contradictory evidence. In some phenomena, such as interference and diffraction, it seems impossible to conceive of it as other than a wave motion in a medium; in others, such as the photo-electric effect, we can conceive of it only as concentrated in particles. Though we have devised mathematical formulae capable of describing both these sets of phenomena, we have not succeeded in framing a verbal description of light that will give us a uniquely clear mental picture of what it is or of how it operates. What, in these circumstances, can we say about the Doppler effect for light?

Historically, the idea that such an effect should exist was suggested by a mental picture of the nature of light (or sound) which was without ambiguity and was believed to be an accurate representation of reality.[1] The source and the observer were connected by a medium in which the waves travelled from the one to the other. First, suppose source and observer to be at rest in the medium. The source emits waves of wave-length λ, and frequency ν, which travel with velocity c. Since ν waves are emitted each second, and each has a length λ, the waves emitted in one second must cover a length $\nu\lambda$. If the source moves towards the observer with velocity V, still emitting waves with the same frequency, then since their velocity is a property of the medium only, the waves emitted at any instant will reach a distance c one second later. But the ν waves emitted during that second will be compressed into a distance $c - V$, since the source will have advanced a distance V beyond its initial point. The wave-length will therefore be reduced to $(c - V)/\nu$. The waves will meet the observer at the velocity c, and the number which will meet him per second will therefore be c divided by this wave-length, i.e. $\dfrac{\nu}{1 - V/c}$; hence he will observe light of a greater frequency than previously. If, however, he also is moving in the same direction, with velocity v, the velocity of the waves relatively to him will be $c - v$, so the frequency of reception, ν', will be this quantity divided by the wave-length, i.e.

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[1] A brief account of the history of the subject by E. N. da C. Andrade appears in *Endeavour*, 1959, **18**, 14.

$$\nu' = \nu \frac{1 - v/c}{1 - V/c} \tag{1}$$

If the motion of source or observer is in the opposite direction to that of the light, the sign of the corresponding velocity will be changed, but the formula will otherwise remain the same. We need not for our purpose consider motion of the medium with respect to some external standard, which, in the case of sound waves in a wind, may be important.

This was the formula which, until the advent of the theory of relativity, was believed accurately to represent the Doppler effect for both light and sound. It is clear that the change of frequency is not the same for a stationary source and moving observer as for a moving source and stationary observer, even when the relative motion of source and observer is the same: in the first case the frequency is $\nu(1 - v/c)$, and in the second case it is $\nu/(1 - V/c)$ and these are not the same when $V = -v$. But if $V = v$ there is no change of frequency, so that the *existence* of the effect depends only on the relative motion of source and observer, although its *magnitude* for the same non-zero relative motion varies with the individual velocities. However, in all applications to light waves until quite recent times, v and V were always small compared with c, and since, to a first approximation,

$$\nu\left(\frac{1 - v/c}{1 - V/c}\right) = \nu\left(1 - \frac{v - V}{c}\right)$$

all that could be inferred from observation was $v - V$, and it was impossible to say anything about the individual velocities of source and observer.

3 *The Doppler Effect for a Particle Theory of Light*

Before considering the effect of relativity on these conceptions it is desirable to see what results would have been reached had a particle, instead of a wave, theory of light been envisaged, for we are now much less confident about the nature of light than were physicists in the nineteenth century. The medium, if any, between source and observer now plays no part in the matter, except to serve as a standard of rest. Clearly, if we are to speak of velocities at all, we must imply such a standard, and we may legitimately assume that it is embodied in a universal medium, or *ether*, so long as we do not grant that medium any physical properties. The frequency, ν, of monochromatic light now becomes the number of particles issuing from the source per second; the velocity of light, c, is the velocity of the particles after emission; and the quantity c/ν will clearly represent the distance between successive particles in the light train. We may represent it by λ and call it, metaphorically, the 'wave-length', since it corresponds in all kinematic respects to the wave-length of our previous considerations.

As before, then, we now suppose the source to move in the direction of the light with velocity V and to emit with the same frequency ν. But now the velocity of the light will change, for, in accordance with Galileo's principle of local motion, each particle will issue with velocity c with respect to its source, and therefore with absolute velocity $c + V$. Each particle will travel a distance $(c + V)/\nu$ before the next is emitted, but the source also will travel a distance V/ν, so that the distance λ between successive particles will be c/ν. Hence the 'wave-length' λ, will not be changed by the motion of the source. The observer, if stationary, will receive the particles at a velocity $c + V$, and the number he will receive per second will be this divided by λ, i.e. $\nu(1 + V/c)$. But if he also is moving, with velocity v, in the same direction as the light, he will receive the particles at velocity $c + V - v$, and the number ν' which he will receive per second will therefore be

$$(c+V-v) \div c/\nu = \nu\left(1-\frac{v-V}{c}\right).$$

This, then, is our general formula for a particle theory of light.

It will be noticed that this is identical with the *approximate* formula for a wave theory, so that for small velocities the two cases are indistinguishable. For large velocities, however, this would have afforded a means of deciding between the theories, if the practical difficulties could have been overcome, but these are so great that the point is of no importance. The really important feature of the formulae is that, on the particle theory, the effect is *entirely* dependent on the relative velocities of source and observer, so that no matter what precision of measurement is attained, the Doppler effect will not, on this theory, distinguish between the motion of the source and that of the observer when they are in relative motion. We may therefore write the particle formula as

$$\nu' = \nu\left(1-\frac{dV}{c}\right), \tag{2}$$

where dV is the velocity of the observer with respect to the source, measured in the direction of motion of the light. This, of course, makes the particle theory compatible with the principle of relativity of motion, and the wave theory, as here considered, incompatible with that principle. But the essence of the difference is not in the nature of light itself, but in the fact that, on the particle theory, the velocity of light has always the same value *with respect to the source*, however that may be moving, while on the wave theory the velocity has always the same value *with respect to the transmitting medium*. To concentrate attention on this distinction, without introducing any assumption about the nature of light, I shall henceforward speak of the 'ballistic theory' and the 'ether theory'. It must be understood, however, that these terms serve only to distinguish the two views concerning the *velocity* of light namely, that on the ballistic theory light always travels at velocity c with respect to the source, and on the ether theory at velocity c with respect to the ether—or empty space if that term is preferred—and do not necessarily imply any of the ideas associated with them at various times in the past. Also, unless otherwise stated, I shall assume that all velocities are constant throughout the whole of the time with which we are concerned.

4 *Intrinsic Properties of Light*

Let us now consider what meaning we are to attach to the three characteristics which we associate with light, namely, *velocity, frequency* and *wave-length*. Velocity (our standard of rest being assumed identified) is a property of the light irrespective of source and observer; for an experimenter at rest, who is unaware that source and observer exist, can time the passage of a pulse of light across a measured distance and so determine its velocity. (Strictly speaking, this is an assumption. I do not know of any experiment in which light is seen to impinge successively on two objects at a known distance apart. It is, however, universally supposed that a pulse of light is 'something' which, at least roughly, may be said to be observably located at the positions of such objects at certain times, and I shall assume that the supposition is justified.) Frequency also is a property of light itself, provided we assume, on the basis of experiment, that light is a periodic phenomenon. It is the number of particles, or wave-crests, or whatever we suppose to give a periodicity to the light, that pass a *stationary* point in unit time. Finally, 'wave-length', if it is defined as velocity divided by frequency, thus automatically becomes an intrinsic property of light, and conceptually, at least, it may be regarded as susceptible of direct determination from the wave-train by assuming a measuring rod to travel with the light. All three quantities, then, may be regarded as characteristics of the light itself, irrespective of how it is produced or received, *provided that we have an objectively defined standard of rest.* If nature does not provide one, and we can choose any one we like for ourselves, then we can vary the velocity and the frequency *ad lib.* by a suitable choice of standard, *but not the 'wave-length'*. However we vary the standard of rest, the quotient of the velocity and the frequency remains constant, so that, if we are to regard light as an objective entity at all, we must grant it only one intrinsic quality, namely, that which we call, literally or metaphorically, the 'wave-length'.

The postulate, or principle, of relativity (i.e. not Einstein's special *theory* of relativity, but the first of the two postulates on which that special theory is based) states that 'the phenomena of electrodynamics as well as of mechanics possess no properties corresponding to the idea of absolute rest'.[1] Light is included in such phenomena, and it therefore follows, if this postulate be accepted, that velocity and frequency cease to be intrinsic properties of light (unless some further qualifying postulate is introduced), and each monochromatic beam is identifiable by its 'wave-length' only. That alone remains the same when we change our arbitrary standard of rest, so that alone characterises the light itself.

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[1] A. Einstein, *Ann. d. Phys.*, 1905, **17**, 891. English translation by W. Perrett and G. B. Jeffery, London, 1923

Let us now look at our Doppler Effect formulae. The ether theory formula is immediately discredited, for its characteristic is that the velocity of light is always the same with respect to the medium, however it may vary with respect to the source. The medium therefore defines an absolute standard of rest, which may be identified by sending out beams of light in opposite directions from a number of bodies in relative motion. The velocities of the beams will in general be $c + V$ and $c - V$ with respect to their sources, and the source for which these quantities are equal will be that for which $V = 0$, i.e. that which is absolutely at rest. This violates the relativity principle, and the distinction has been found practically impossible since experiment gives $V = 0$ for all sources, so we need consider only the ballistic theory formula (2).

We have already noticed that this conforms to the relativity principle in that it involves dV but not V and v individually. Further, it determines ν'/ν, but not the individual values of ν' and ν. This agrees with our general considerations, but it is to be noted that ν' and ν may have definite determinable values because they are not properties of light but the *frequencies of reception* of light by the observer. If we suppose the observer at rest and source moving with velocity V, then the light travels at velocity $c + V$ and with a frequency $(c + V)/\lambda$, and if we suppose the source at rest and the observer moving, the light travels at velocity c and with a frequency c/λ. The frequency of the light itself varies with our assumption, but the frequency of reception is the same since the relative velocity of light and observer is, on either assumption, $c + V$, and the frequency of reception is therefore $(c + V)/\lambda$. When the source and observer are relatively at rest, $V = 0$, and the frequency of reception, ν, is c/λ.

To sum up, then, we see that if we postulate the existence of a periodic phenomenon called light, we can assign to it one single intrinsic property which we may call 'wave-length' (λ) and two indefinite properties, which we call its 'velocity' and 'frequency', which vary with our arbitrary choice of a standard of rest: their ratio is equal to λ, but individually they are undetermined. We can, however, determine each of them with respect to a chosen standard of rest. In that case the velocity on the ballistic theory is always c with respect to the source and $c + dV$ with respect to the receiver, while the frequency of emission is c/λ and that of reception $(c + dV)/\lambda$.

5 *Test of the Ballistic Theory*

This is essentially the theory put forward by W. Ritz in 1908,[1] though he did not apply it to the Doppler effect. It has for long been regarded as disproved, chiefly because of a demonstration by de Sitter that any theory that requires the velocity of light in space to depend on the velocity of the source at the time of emission would demand apparent motions of the components of binary stars which do not agree with observation. de Sitter's argument is sound, on the basis of his assumptions, but it does not dispose of the ballistic theory.[2] The velocity of light and of the source must, on the relativity principle, be related to some standard of rest, and de Sitter tacitly chose the Earth as such a standard. But if, in accordance with the ballistic theory as here defined, we choose the emitting body, theory and observation agree perfectly. Strictly speaking, this phenomenon lies outside Ritz's considerations, for the relative velocity of the Earth and star is not constant, and its variation is an essential part of the test. But if we generalise the postulate of relativity, which Ritz accepted for uniform motions, to motion of all kinds (implying not necessarily Einstein's 'general theory of relativity', but only the postulate that any body, in whatever state of motion it may be, may be regarded as being at rest), then we would expect the velocity of light *with respect to its source* to remain constant, and not its velocity with respect to an arbitrarily chosen body such as the Earth. In that case the phenomena cited by de Sitter would be quite consistent with the ballistic theory.

A necessary implication would be that if the velocity of emission of the light were c with respect to the star, its velocity of reception at the Earth would be $c + dV$, where dV is the velocity of the star with respect to the Earth in the direction of travel of the light. The only practicable way of measuring the velocity of reception of light from a distant inaccessible source is to make use of the phenomenon of aberration. The direction in which a star appears is determined by compounding the velocity of its light with that of the orbital motion of the Earth with respect to the Sun. Since the latter motion varies continuously in direction, the star appears to describe annually an ellipse in the sky, with characteristics that are well known and agree with theory within the limits of observational error. The stars have various motions with respect to the Earth, as is shown by their ' proper motions ', and on Ritz's theory their light should reach the Earth at various velocities, and so their aberrational ellipses should differ. All such variations, however, are too small to be detected. But with the extra-galactic nebulae it may be otherwise. These show spectrum shifts which, if they are correctly identified as Doppler effects, indicate that distant nebulae are receding at velocities which should ensure quite unmistakable changes in the aberrational ellipses.

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[1] W. Ritz, *Ann d. Phys. et de Chim.*, 1908, **13**, 145

[2] See H. Dingle, *Mon. Not. R.A.S.*, 1959, **119**, 67

At my request, Dr I. S. Bowen, Director of the Mount Wilson and Palomar Observatories, has very kindly agreed that examination shall be made of the positions of such nebulae at different times of the year. The result is not yet available, but earlier independent observations by Strömberg, von Biesbroeck and Heckmann,[1] of a cluster of nebulae not very suitable for this purpose, show no appreciable difference between the aberrational constants for the nebulae and for stars in our galaxy. If this result is general, the conclusion is that if the spectrum shifts are, in fact, Doppler effects, and if the accepted theory of aberration of light is correct, the light from these nebulae is received at the Earth at velocity *c*, identical (within the limits of observational error) with the velocity of light as measured in terrestrial laboratories. There seems no reason to question the theory of aberration, but the interpretation of the spectrum shifts is much more doubtful, as we shall see. At present, therefore, we can only say that the question is open. It would be settled definitely by a laboratory comparison of the velocities of light from stationary and moving sources.

6 *Einstein's Special Relativity Theory*

A few years before Ritz's paper appeared, Einstein put forward his ' special relativity theory ', which added to the postulate of relativity a second postulate, the ' postulate of constant light velocity ', namely, ' that light is always propagated in empty space with a definite velocity *c* which is independent of the state of motion of the emitting body '.[2] As so stated, the postulate is equivalent to the ether theory, which we have seen to be inconsistent with the postulate of relativity, for we have only to substitute 'the transmitting medium' for 'empty space' to make it a requirement of that theory. Kinematically these phrases are synonymous, and we are not concerned with any physical properties that might distinguish them. Einstein, however, stated that his new postulate was 'only apparently irreconcilable' with the relativity postulate. He added that 'the introduction of a "luminiferous ether" will prove to be superfluous inasmuch as the view here to be developed will not require an "absolutely stationary space" provided with special properties, nor assign a velocity-vector to a point of the empty space in which electromagnetic processes take place'.

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[1] See *The Observatory*, 1959, **79**, 105

[2] Einstein, op. cit.

Literally, these statements are clearly contradictory. You do not assign a velocity-vector to a point of empty space, but the velocity of light in empty space is c. Velocity with respect to what? The development of the theory shows that it means 'velocity with respect to anything at all'. You do not assign a *unique* velocity-vector to a point of empty space, but you can choose any standard of rest you like, and the velocity of light with respect to it will always be c. It is impossible to picture this, for if we have two bodies in relative motion, we cannot picture a third body having a finite velocity which is the same with respect to both. We must content ourselves with stating the result of an experimental measure of the velocity of any beam of light with respect to any body at all. The theory therefore demands that we give up the attempt to picture, not merely what it is that moves, but the process of motion itself, the passage from point to point as time goes on. That requires us to express in a formula the velocity of light with respect to two relatively moving bodies in such a way that the value c results for both.

The theory satisfies this requirement. The velocity of emission of light from any body, however it is moving, is measured as c with respect to the body—our limited experience on Earth conforms to this. At what velocity will that light be received at the Earth if the body is moving towards the Earth with velocity dV? According to the theory the answer is

$$\frac{c + dV}{1 + \frac{cdV}{c^2}} = c.$$

Hence, no matter what value dV may have, the light will be received at velocity c. Moreover, this is not a peculiarity of light. Anything else, moving with velocity c, would show the same phenomenon. Nor is the velocity c the only velocity that defies our attempt to picture it. If something were emitted with velocity U, whatever U might be, its velocity of reception would be

$$\frac{U + dV}{1 + \frac{UdV}{c^2}}.$$

The theory forbids us to form a picture of *any motion at all.* We can visualise the setting out and the arrival of a travelling body: what happens in the empty space between is incapable of precise expression. We can make an approximate image of it for small velocities; that is all.

Let us, then, now review our three theories with respect only to the velocities of emission and reception of light with respect to source and receiver, respectively. On the ether theory the light is emitted at velocity $c - V$ and received at velocity $c - v$. On the ballistic theory it is emitted at velocity c and received at velocity $c - dV$. On Einstein's theory it is emitted at velocity c and received at velocity c. The ether theory violates the postulate of relativity, but satisfies the postulate of constant light velocity by distinguishing the 'real' velocity of light from its measured velocities of emission and reception. The ballistic theory satisfies the postulate of relativity, but violates the postulate of constant light velocity. Einstein's special theory of relativity satisfies both postulates. The weight of experimental evidence against the ether theory (which includes, among other things, all experiments of the Michelson-Morley type) is very great. Experiment has not yet finally decided between the ballistic theory and Einstein's theory.

7 *Relativity and the Doppler Effect*

Now let us return to the Doppler effect, and see how our conclusions are affected by the advent of the relativity theory. The first thing to notice is that, contrary to what seems to be a general belief, there is no necessary relation at all between the relativity theory and the Doppler effect. Of course, if we have independent reasons for supposing that there is such a thing as a Doppler effect, then the relativity postulate requires that, like all other phenomena, it must not enable us to distinguish in an absolute sense between the motion of the emitter with respect to the receiver and that of the receiver with respect to the emitter. But it would be entirely consistent with Einstein's theory if there were no Doppler effect at all—i.e. if motion had no effect on the observed frequency of light—or if the relation of frequency to velocity followed any of the infinite number of formulae satisfying the relativity postulate. The postulate of constant light velocity speaks only of the *velocity* of light: it does not require that light shall even show a periodicity. That we infer quite independently from experiment, and therefore the Doppler effect formula can have only an empirical basis, or possibly a theoretical basis in some quite independent theory.

Suppose, however, that we take light to be a periodic phenomenon, and accept the relativity postulate that nature provides no absolute standard of rest. We can then dismiss the ether theory as incompatible with the latter, and consider only the ballistic theory and Einstein's theory in an attempt to derive a *theoretical* Doppler formula. On the ballistic theory the matter is simple. The emitting body sends out something (particles or waves or anything periodic we like) at the rate of ν per second. These entities fall on the receiver at a velocity $c - dV$, so the frequency of their reception must be $\nu \dfrac{c - dV}{c} = \nu \left(1 - \dfrac{dV}{c}\right)$. This is our formula (2), which already conformed to the relativity postulate and so required no modification. If A radiates to B and B to A, each will observe the other's spectrum displaced in the same sense and by the same amount, no matter how their distance apart is changing. It remains a matter for experiment to verify that this is so and to determine whether formula (2) does in fact give the right relation. The necessary experiments have not been made, but we shall consider presently what experimental evidence we have bearing on the question.

On Einstein's theory we have no basis for assuming any formula at all. The passage of light is unpicturable, and we can form no conception of what frequency can mean when applied to the encounter of an observer with something indescribable. Nevertheless, a formula has been derived—Einstein derived it in his first paper on the subject.[1] In the simple case, which alone we need consider, in which the two bodies concerned are directly approaching or receding from one another, Einstein's formula is

$$\nu' = \nu \sqrt{\frac{1 - dV/c}{1 + dV/c}} \tag{3}$$

This clearly satisfies the relativity postulate, and, as we have seen, necessarily satisfies the postulate of constant light velocity since any such formula will do that, velocity and frequency having been entirely divorced from one another.

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[1] Einstein, op. cit.

But how, in view of such divorce, is a formula obtained? It was actually first derived by assuming the existence of light as a system of electromagnetic waves, and then applying the mathematical transformations required by the theory, whence two frequencies were obtained, related according to equation (3), and identified with observed frequencies of light from the two relatively moving bodies. But Einstein, Lorentz, and other theoretical physicists have always held that the mathematical transformations correspond to actual physical phenomena—for instance, that the change of length, L, into

$$L\sqrt{1 - V^2/c^2}$$

when the co-ordinate system is changed, represents an actual physical contraction of a rod when it changes from a state of rest in the first system to a state of rest in the second. Accordingly, if we are to relate the mathematics to actual physical events, we must express the Doppler spectrum shift in terms of physical characteristics of the source of light, the light itself, and the observer's apparatus.

But this appears to be impossible. The nearest we can get to it is the following. Let us make two assumptions. First, let us assume that light consists of a periodic structure which issues from the source with velocity c and, although meeting the observer with velocity c also, nevertheless meets him with the frequency which would be expected if the velocity were $c - dV$, as in the ballistic theory. Then, if the frequency of the emitted light is ν, that of the light reaching the observer will be $\nu' = \nu(1 - dV/c)$. The second assumption is that all periodic phenomena are such that their frequencies must be regarded as reduced, if they are moving with velocity dV, by the factor $\sqrt{1 - dV^2/c^2}$. Now the observer is moving towards the source with velocity $-dV$. Hence the frequency of the light which he emits is $\nu_0 = \nu\sqrt{1 - dV^2/c^2}$ where ν is the frequency which would have to be assigned to his light, if, like the source, he were at rest. Accordingly, the ratio of the frequency which he receives from the source to that which he emits from his own lamp is $\nu'/\nu_0 = \sqrt{\dfrac{1 - dV/c}{1 + dV/c}}$.

These assumptions are not only compatible with Einstein's theory; they have actually been made in the course of its development, and they yield the expected formula. The formula has the required characteristic that it gives opposite frequency changes for approach and recession, and is a function of the relative velocity of the bodies only. But this view of the matter compels us to assume that the observer, and not the source of light, is the moving body. If the source moves and the observer remains at rest, it requires us to put

$$\nu' = \nu\left(1 - \frac{dV}{c}\right)\sqrt{1 - dV^2/c^2}$$

and $\nu_0 = \nu$, so that $\nu'/\nu_0 = \left(1 - \dfrac{dV}{c}\right)\sqrt{1 - dV^2/c^2}$. The two ways of regarding the motion are thus not equivalent, and the postulate of relativity is violated. No other physical justification of the formula has ever been suggested, as far as I know, so if Einstein's theory is to be maintained we must add to the impossibility of picturing light as a moving something, a similar impossibility of picturing it as a periodic phenomenon.

In this situation the odds seem heavily in favour of the ballistic theory. Indeed, on Einstein's theory we have no reason to suppose that there is any such thing as a Doppler effect for light, even if we allow light to be periodic. It will be remembered that the evidence for the effect came originally entirely from the ether theory of light coupled with the ordinary addition law for the composition of velocities. The ballistic theory discards the ether, but still requires a Doppler effect, though with an altered formula. But Einstein's theory discards both ether and velocity addition formula, and leaves nothing to suggest that what they imply will still have significance. The natural inference, if Einstein's theory is accepted, is that the Doppler effect is a mere speculation based on an erroneous theory, and that no corresponding phenomenon is to be expected.

8 *Observational Evidence for the Doppler Effect*

This simple conclusion, however, is not now possible, for we have *observational* evidence that the effect exists. We know from sunspot observations that the Sun rotates on an axis, and the spectrum of light from opposite limbs shows displacements of the right character and roughly proportional to the respective velocities. Concordant results are obtained from the light reflected by rotating planets. There are a few binary stars, whose orbital motion is observed in the telescope, which show corresponding spectrum shifts. Terrestrial experiments, made with light reflected from moving mirrors give similar evidence. In all these observations the receiver is regarded as being at rest, but the orbital motion of the Earth is accompanied by oscillatory spectrum shifts in the light from stars, so we may say that the Doppler effect has been shown to exist when one body approaches or recedes from another, no matter which, on general grounds, we find it convenient to suppose is the moving one. We have no direct evidence of the *reciprocity* of the effect—i.e. simultaneous evidence that if A observes B's light to be displaced with motion, B will observe A's light to be displaced in the same sense and by the same amount. It would be highly desirable to test this, but the required observations are difficult, though perhaps not impossible in rocket experiments. In none of the observations just cited can the measurements be made precise enough to distinguish between formulae (2) and (3). In the only known astronomical phenomenon giving sufficiently large displacements—the so-called recession of the galaxies—we have no independent evidence that the galaxies are in fact receding. Indeed, we have reason to believe that in at least one other physical situation—the presence of a gravitational field—a spectrum shift is produced which is indistinguishable (so long as observations from both bodies are impossible) from the Doppler effect, although the bodies concerned may be relatively stationary. We must, therefore, as already remarked, at present suspend judgment on this point.

The most suggestive experimental evidence, however, comes from a celebrated experiment by Ives and Stilwell,[1] who observed the radiation from hydrogen atoms in a beam of canal rays which had been made to move at a high velocity by the action of an electric field. By inserting a mirror in the tube they were able to observe together, on the same plate, the radiation from rapidly approaching atoms and, in effect, from their equally rapidly receding reflections in the mirror. They accordingly obtained two spectrum lines displaced by large amounts (up to 32A. in extreme cases), one on each side of the normal position of the same radiation. They could estimate the speed of the atoms from the characteristics of the applied electric field and the equations of electromagnetic theory, and the displacements agreed roughly with those to be expected from equations (2) and (3). From their observations they concluded that Einstein's formula (3) was the correct one.

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[1] H. E. Ives and G. R. Stilwell, *J.O.S.A.*, 1938, **28**, 215

The difference, however, between the formulae for each individual displacement is so small, and the normal position of the spectrum line from a stationary atom under the conditions of the experiment is so doubtful, that this conclusion must be regarded with considerable reserve. The observations, however, do afford evidence of much greater strength. Whatever the velocities may have been, it is hardly questionable that if the velocity of the approaching atoms was V, that corresponding to their reflected radiation was $-V$. Now according to formula (2) of the ballistic theory, the frequency for a stationary source is the *arithmetic* mean of the frequencies for two velocities, V and $-V$, while according to formula (3) of Einstein's theory, it is the *geometric* mean of those frequencies. Consequently, we can by-pass our uncertainty about the frequency for a stationary source under the conditions of the experiment, by seeing which mean value is the same for all velocities.

The data published by Ives and Stilwell are rather meagre, but the records of their experiments are now in the possession of Dr Pascal M. Rapier, President of the Astronomical Society of Nevada, who has kindly supplied me with a considerable selection of observations, in which the estimated values of the velocities range from 640 to 2,000 km./sec. These leave no doubt that it is the geometric mean, and not the arithmetic mean, that is independent of velocity—indeed, to a degree of accuracy that one would hardly have supposed the experimental arrangements to allow. If, then, these results are to be accepted—and, especially in view of compatible observations by G. Otting,[1] no reason appears for doubting them—we must accept it as a fact that formula (3) of Einstein's theory gives the observed relation between spectrum shift and theoretical velocity. (I say 'theoretical' velocity because it is assumed that the equations of the electromagnetic field yield velocities strictly equivalent to measured distance divided by measured time. As we shall see later, there is some reason to question this.[2] However, the point is unimportant here, for even if the quantity here called V is not the true velocity, the latter must be such a function, $f(V)$, of V that $f(V) = -f(-V)$, and what has been said about the arithmetic and geometric means remains unchanged.)

It appears, then, that on experimental grounds we must accept formula (3)—with the possible substitution of $f(V)$ for V—as giving the correct relation between velocity and spectrum shift. It is true that other formulae are possible that would satisfy the evidence—the frequency could, in fact, be any function, $\phi(V)$, of V obeying the relation $\phi(V) \times \phi(-V) =$ constant—but no other has ever been suggested, and our problem really consists in reconciling the apparent inevitability of the ballistic theory, which gives what appears to be the wrong formula, with the apparent impossibility of the Einstein theory, which gives what may be the right formula. I shall therefore assume that formula (3) agrees with observation.

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[1] G. Otting, *Phys. Zeits.*, 1939, **40**, 681

[2] See H. Dingle, *Bull. Inst. Phys.*, 1958, **9**, 314

The crucial experiment, as before, would be the determination of the velocity of light from a moving source on the Earth—preferably over a one-way journey, but if we are justified in assuming that light is reflected from a mirror with the velocity of incidence, a two-way journey would be satisfactory. And here it may be said that a one-way determination is by no means theoretically impossible, as is often stated. The synchronization of separated clocks by light signals presupposes nothing about the magnitude of the velocity of light: it merely defines time at a distance in such a way as to make the forward and backward journeys of the light take equal times when the clocks are relatively at rest. An experiment is perfectly conceivable in which two bodies, A and B, relatively at rest at a distance X apart, carry clocks which are synchronized by light-signals. A third body, C, moving at high velocity with respect to A and B, travels from the former to the latter, and at the instant of its coincidence with A, both A and C send out light pulses towards B. Einstein's theory is built on the assumption that these signals would reach B at the same instant: on the ballistic theory the signal from C would arrive first. The times of their arrival could be observed on the clock at B, and therefore the velocities of the two beams in the A,B rest-system could be calculated. But even without an actual measurement of the velocities, the correctness of Einstein's postulate could be tested by the fact of their simultaneous or successive arrival. It is very greatly to be hoped that such an experiment as this will be attempted if possible.

9 *The Theories Contrasted in Relation to the Doppler Effect*

In the meantime, however, let us examine the consequences for the Doppler effect of assuming, in turn, each of the possible results to be realised. First, suppose the ballistic theory to be corroborated, i.e. the light issues from the source at velocity c with respect to the source, and reaches the receiver at velocity $c - dV$ with respect to the receiver, where dV is the velocity in the direction of the light, of the receiver with respect to the source. Then it is inescapable that the relation between the frequency of reception ν', of the light elements (particles, wave-crests, . . .), and the relative velocity, dV of source and observer is given by (2), viz. $\nu' = \nu(1 - dV/c)$. On the other hand, the Ives-Stilwell experiment requires us to conclude that the relation between the spectrum frequency, N', and the relative velocity of source and observer is $N' = N\sqrt{(1 - dV/c)/(1 + dV/c)}$. Here by 'spectrum frequency' I mean the quantity, commonly called the frequency of the light, which is derived from spectrum measurements, and N is the spectrum frequency when source and observer are relatively at rest. It follows that $\dfrac{N'}{N} = \dfrac{\nu'}{\nu}\dfrac{1}{\sqrt{1 - dV^2/c^2}}$. This is consistent with the general relation

$$N = \frac{A\nu}{\sqrt{1 - dV^2/c^2}}, \tag{4}$$

where A is a constant, and the primes have been omitted for convenience, N, ν corresponding to a relative velocity dV between source and observer.

This leads us to inquire into the physical meaning of the quantity which we derive from the spectrum and call the 'frequency' of the light. It is usually, perhaps too naively, supposed to be identical with ν, and in a passage in section 7 we have implied this identification. But a little reflection shows that the matter is much more complicated than that. Let us take the simplest—and at the same time the most fundamental—evidence of the existence of a spectrum frequency. Consider monochromatic light incident normally on a Fabry-Perot interferometer, which consists of two parallel, half-silvered plane glass plates separated by a small empty space. The light that emerges on the other side consists of a number of beams whose paths of travel in the instrument differ successively by equal finite lengths. One beam goes straight through; another is reflected to and fro once before transmission; another twice, and so on. If the distance between the plates is e, the paths are therefore e, $3e$, $5e$, . . . What is observed, with a certain arrangement of the apparatus, is a number of concentric narrow rings of light on a dark background. If, now, we gradually change the distance between the plates, all the rings expand at the same rate and others appear to open out from the centre, preserving the general pattern. When the distance has changed by a certain small amount, *de*, the original pattern is exactly restored, each ring having moved out into the place previously occupied by its neighbour. A change by 2*de*, 3*de*, . . . restores it again, so we have evidence of a *periodicity* in the phenomenon, the period being related in some way to the distance *de*—which corresponds, of course, to a difference 2*de* between the paths of neighbouring beams in the set that combine to give the pattern.

We have been accustomed to explaining this by supposing that the light-train has a structure that repeats itself every 2*de* units of distance, and we call 2*de* the *wave-length* of the light and represent it by λ. If we change to light of a different colour, 2*de* has a different value, and so we say that beams of light of different colours have different wave-lengths.

But clearly this will not do. If there are two such interferometers side by side, receiving light from the same source, they of course yield the same ' wave-length '. If, now, one moves towards the source, the light is not changed, as observations with the other will prove, but we obtain a different value of *de* with the moving one. Accordingly, *de* cannot measure a property of the light, and we know that λ is a property of the light.

The same thing is seen, perhaps even more compellingly, by considering two sources of different kinds of light, at rest side by side; to fix our ideas, suppose they emit the hydrogen red line at λ6563A. and the lithium red line at λ6708A. When our interferometer is stationary we obtain a certain value for *de* with the hydrogen line. When it moves towards the sources, a particular velocity is reached at which we get this same value of *de* for the lithium line. *All* the indications of our interferometer, and indeed of any kind of spectroscope at all, will be exactly the same for the light from the two sources under these conditions. Hence, quite apart from all theories, *de* cannot measure the wave-length of light, however ' wave-length ' be interpreted.

But if the spectrum does not measure a property of the light, what does it measure? It can only be some characteristic of the interaction of the light with the apparatus, which changes with their relative motion. Let us, then, put $2de = f(\lambda, dV)$, where λ is a property of the light alone and dV is a property of the source-receiver system alone. If N be the spectrum frequency, then, by definition, $2de = c/N$, so that

$f(\lambda, dV) = c/N$. Now we have seen that the Doppler formula (3) requires, on the ballistic theory, that $N = \dfrac{A\nu}{\sqrt{1 - dV^2/c^2}}$, so that $2de$ on that theory must be $\dfrac{c\sqrt{1 - dV^2/c^2}}{A\nu}$. The wave-length λ, is, as we have seen, a property of the light, independent of its relation to source or receiver, and is equal to $(c - dV)/\nu$, i.e. the ratio of the velocity and frequency of reception. Hence

$$A.\,2de = \lambda\sqrt{\frac{1 + dV/c}{1 - dV/c}} \qquad (5)$$

I do not know of any means by which this relation can be tested, since we have no practicable means of measuring λ; all our methods of measuring what we call the wave-length of light measure *de*, and not λ, and may give, as we have seen, the same value for kinds of light which we know have different 'wave-lengths', whatever 'wave-length' might mean. Nor have we sufficient knowledge of the structure of light and the process by which it interacts with the interferometer to derive a theoretical relation between *de*, λ and *dV*. We can only say that if formula (3) is correct, then on the ballistic theory this relation must hold.

Now let us turn to Einstein's theory, and see what interpretation it gives of the change in *de* when the interferometer changes from a state of rest to a state of approach with respect to the source of light. The light itself does not change, so λ does not change. By hypothesis, the relative velocity of the light and the interferometer does not change. Hence the frequency of reception of the light, which is the quotient of velocity of reception and wave-length, does not change. But *de* does change. There is only one possibility: the dimensions of the apparatus must change with motion, so that an adjustment must be made to restore the original pattern. But if this be the explanation, the apparatus must expand when moving in one direction and contract when moving in the other, for *de* changes in opposite senses in the two cases, and this is contrary to the general requirements of Einstein's theory concerning the effect of motion on the size of a body. We must accordingly reject this supposition, and admit the phenomenon to be inexplicable.

We are once more thrown back on the complete unpicturability of the physical processes occurring in these experiments, and compelled to acknowledge the theory as a calculating device, predicting what we shall observe in given circumstances, but yielding only contradictory ideas if we try to form a physical conception of what is taking place. We are familiar with this kind of situation in quantum theory, but there the entities whose location or motion is inapprehensible are themselves purely conceptual; we have not previously encountered such an anomaly in macroscopic physics. But this does not necessarily condemn Einstein's theory. In the last resort, a theory fulfils its purpose if it does in fact agree with observation at all points; it need not have anything to say about what happens between the observations. At present both the ballistic theory and Einstein's theory satisfy this basic requirement, so far as they have been tested, and although on aesthetic grounds the former is the more attractive, a decision can be made between them only by such an experiment as that already mentioned or else by the detection of inconsistencies in one of them. We shall return to this point.

(*To be concluded.*)

SIMPLICITY IN SCIENTIFIC THEORIES *

H. R. POST

THE term simplicity is often invoked in connection with scientific theories.[1] Yet there are few technical discussions of it.

The merit of a scientific theory is judged not only by its logical consistency, and its correspondence with experience. The absence of logical contradictions and the absence of empirical evidence contradicting statements of the theory are not the only criteria for acceptance of the theory. (Indeed many crank theories, precisely the ones most difficult to eliminate, would qualify for acceptance under these two criteria.) There are, of course, grave problems attaching to these first two criteria. But the remaining ground on which we judge a theory is certainly less well defined still. There is, even now, a widespread feeling that it could not, or should not, be more closely defined, that it is essentially an aesthetic criterion of 'elegance' or 'simplicity', and that such a criterion is ultimately subjective in application. This is not the view of this paper. The establishing of objective standards in this field is not only of academic interest. It might well prove to be of practical value.

We shall refer to the remaining criterion of merit of a theory as

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* Apart from minor changes, this is a paper read at the Philosophy of Science Conference, Oxford, September 1958.

[1] '. . . it will always be possible thus to reformulate a law, postulated at first only for certain co-ordinate systems, such that the new formulation becomes formally universally covariant. Beyond this it is clear from the beginning that an infinitely large number of field-laws can be formulated which have this property of covariance. The eminent heuristic significance of the general principles of relativity lies in the fact that it leads us to search for those systems of equations which are *in their general covariant* formulation the *simplest ones possible*; among these we shall have to look for the field equations of physical space.' (Albert Einstein, 'Autobiographical Notes', *The Philosophy of Albert Einstein*, Evanston, 1949 p. 69.)

'. . . for the prediction of an unknown relation between physical quantities, the common method of dimensional analysis has no logical or mathematical basis. If we employ it in the hope that it may lead to useful results, this hope must be based not upon any established principles of science, but upon some vague belief in the simplicity of nature.' (G. N. Lewis, 'Ultimate Rational Units', *Philosophical Magazine*, 1925, **49**, 750.)

' simplicity '.[1] This is without prejudice to the possibility that several, more or less independent, criteria may be involved. In fact, we shall find two closely linked criteria (' linguistic ' and ' semantic simplicity ') and one criterion (' pragmatic simplicity ') that is, at least logically, independent of the other two.

It should not be necessary to emphasise the *importance* of clarifying the notion of ' simplicity '. The issue arises in education, where ease of understanding due to familiarity on the basis of particular previous school training is confused with technical simplicity inherent in one theory rather than another. The confusion is analogous to the confusion of ' obvious ' in mathematics in (*a*) the psychological and (*b*) the technical sense.

There is a more ambitious use to which, one might hope, a well-defined measure-of-simplicity might ultimately be put: the elimination of certain classes of crank theories or, more generally, the relative grading of theories according to merit.

Most ambitious of all: a clarification of the notion of simplicity might be of heuristic value in constructing new theories.

Finally, there is the old question whether simplicity in science is entirely a tautological result of scientific activity, or whether the universe is simple in some objective sense. This question is answered here by reference to one type of simplicity.

There is, in some cases, some measure of agreement in the judgment of simplicity between different judges: We feel one theory to be unnecessarily complicated, clumsy, perhaps an *ad-hoc* theory, while another theory is more elegant in covering the same field.

In spite of our conviction that one theory is simpler than another, we nevertheless immediately run into a difficulty.

The most obvious approach to measuring the simplicity of a theory would be to base our judgment of simplicity solely on the linguistic form of the theory. But this will clearly not do: the linguistically simplest possible statement is $X \equiv 0$, and *any* theory can be cast into this form by suitably loading the definition of X.[1] On the other hand, I claim that the question of the linguistic form of a theory is far from being trivial, either: two scientific statements may cover precisely the same range of facts, have the same empirical content, and yet may differ significantly in merit as theories or building stones of theories.

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[1] The choice of name implies, of course, the view that there is considerable overlap between the notion of simplicity in common usage, and the ' third criterion ' discussed here. As we shall see, it covers such, perhaps at first sight diverse, but actually linked, ideas as ' brevity of statement ', ' strength ', ' falsifiability ', and ' fruitfulness ' of a theory.

Criteria of simplicity, logical consistency, and agreement with experience, are put forward as jointly necessary and sufficient in judging a theory.

To give some examples[2]: (1) Consider the finite number of lines of the hydrogen spectrum known at the time of Balmer's discovery. We might compile a list of the frequencies. Compare this with Balmer's statement of the *same* frequencies:

$$\begin{array}{l} R(1/2^2 - 1/3^2) \\ R(1/2^2 - 1/4^2) \\ R(1/2^2 - 1/5^2) \\ R\,1/2^2 \end{array}$$

Even if Balmer's law did not imply an infinity of lines, I claim that it already presents a simplification in the representation of the finite number of lines.

Incidentally, there are cases where the number of data is *essentially* finite, such as the 'radii' of the planetary orbits. Bode's law is a simplification over a mere list of the radii.

(2) Nor need the data be given with infinite (or even *present*) experimental accuracy by the simple law. It was shown that, while Prout's hypothesis is not strictly true, nevertheless the random chance of atomic weights being as near to whole numbers as they are is only one in a hundred. Thus Prout's hypothesis is a worth-while simple, though approximate, statement over the finite domain of known elements.

(3) Usually simplification does mean reduction to whole numbers. Nevertheless there are examples of simplification in the continuum:

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[1] We might, for instance, define X as curl $\mathbf{H} - 4\pi \mathbf{j}/c$, thus reducing Maxwell's equation for the case of steady currents to the form $X \equiv 0$. Again we might spuriously 'unify' two field theories (such as the theory of electro-magnetism on the one hand, and of gravitation on the other), by defining a new field with correspondingly more components. As long as these components are completely separate, this is, of course, a trivial linguistic dodge. These examples are only intended to point up the necessity for a criterion less arbitrarily based on a particular language. (I differ from Popper, *The Logic of Scientific Discovery*, London, 1959, p. 143, in considering that we must explicitly guard against classes of what are there called 'irrelevant' co-ordinate transformations.)

[2] Examples are taken from the physical sciences; the biological and psychological sciences add special problems not all of which are relevant to the discussion at this stage.

Considering the solar system only, there is no empirical difference between Copernicus's system and Tycho Brahe's last system. Nevertheless the heliocentric system undoubtedly represents a simplification linguistically, by a convenient choice of co-ordinate system.

This kind of simplification makes the information at the same time more easily remembered.[1] The simpler statement is easier to remember. 'A good theory is a good mnemonic'.

These examples show that there is already some significance in ordering theories according to their simplicity entirely in the linguistic field, introducing the notion of Linguistic Simplicity, Simplicity I. This type of simplicity will be related in this paper to the better-established Simplicity in the semantic field, Simplicity II, Semantic Simplicity.

This could be described as the *power* of a theory. We identify at least two types:

(*a*) Two theories may differ in generality: one theory may state conservation of momentum for elastic collisions, another, simpler one, may state conservation of momentum for any system. The latter specifies fewer conditions to be fulfilled.

Now here we have one type of simplicity, at any rate, that we can point out as an 'objective' feature of nature: we may formally describe any scientific law as the statement of a correlation between some parameters under certain conditions.[2] Thus there is a correlation between an electric current (measured electrochemically) in a wire and a magnetic field produced by it in the air surrounding it. Now one example of the strength of the law expressing the proportionality between current and field is that it holds whatever metal may be used for the wire. 'The chemistry of the wire is an irrelevant parameter'. This fact is not a matter of convention. The usefulness of a law depends on the vast range of parameters irrelevant to it.

Many features of our universe can be described (within certain limits and to a certain accuracy) by laws that are simple in this sense. This is not a matter of convention (we might imagine a completely chaotic universe in which no laws would be possible, though we could hardly exist in such a universe). We summarise this aspect of simplicity in the statement:—*the irrelevance of the values of some parameters to the invariant correlation of some other parameters is* not *a matter of convention.* The more parameters can be ignored, the greater the simplicity of the law.

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[1] This remark is intended to point out a parallelism between simplicity (as exemplified above) and suitability for memorising, *not* to offer a psychological definition of simplicity.

[2] It seems that argument arises even on this point. It would lead too far to do more than state my view that this covers predictive laws which I do not consider essentially distinct from laws expressed in other forms.

(*b*) We may also grade with respect to simplicity laws showing different dependence on a given parameter: a linear relationship does appear to be simpler than one depending on the second power, etc. Circular orbits for planets are simpler than elliptical ones.

Popper[1] has shown how his criterion of falsifiability covers both types (*a*) and (*b*) of (what is here called) semantic simplicity. A theory is the simpler, the more easily it could be falsified. The degree of falsifiability depends on the number of parameters that have to be fixed in order to make it logically possible for the theory to be falsified. The simpler theory is richer, more powerful, it says more.

If the reader agrees with the preceding qualitative discussion of what we mean by simplicity, we can now proceed to outline the principles which allow a measurement of simplicity. The first step is to establish a relation between Simplicity II and Simplicity I. We shall show that they are complementary: we may think of a theory as carrying a certain message, having a certain semantic content. Two theories may differ in semantic content. If they disagree, empirical tests may decide between them, though we may assign relative simplicities to the two theories even before then. Suppose, however, we consider two theories (having the same range of application) at such a stage that they do not contradict each other: one theory may state that planetary orbits are elliptical, while the other, simpler, one states that they are circular. The latter is simpler because four position determinations could falsify *it*, but not, in general, the former. We may, however, graft *ad-hoc* additions to the poorer theory so as to make it semantically equivalent to the richer, simpler, theory. For instance, in the case of elliptical orbits, we may specify the ratio of major to minor axis length to be unity.[2]

By such ad-hoc *additions we can reduce the discussion of simplicity to a linguistic one, semantic and linguistic differences being alternative aspects in comparing two theories.*

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[1] Popper, op. cit., Chapter 7

[2] A similar procedure applies in case (*a*): we may consider the poorer theory to allow a very general functional dependence on certain parameters, while the stronger, simpler theory specifies the function to be a constant.

In order to compare the linguistic simplicity of two theories carrying the same message we just measure the length of the message in the two cases. For this we need a common basis-language, which raises a problem we will consider in a moment.

But given a basis-language, any theory can be regarded as a code message, presenting a message in the basis-language in a highly condensed form by a system of coding. Two semantically equivalent theories are obtained from the same message in the basis-language, by different systems of coding. *The problem of finding the simplest theory is the problem of optimum coding.* The procedure of coding in this sense is discussed in a paper on memory mechanisms.[1] The author considers the problem of economy in storage of information. The procedure is as follows: Any repeated pattern in the original message is noted. If it occurs sufficiently frequently, it is worth while to give a code symbol to that pattern, and to re-cast the message into the coded form, together with a statement of the code back into the original language. Repeated coding may be indicated.

This is essentially a procedure in constructing theories: We note recurrence in overall change, we find invariants and give them a name. There are no emergency baptisms in science. We only give a name to a quantity if we think it is going to survive, to stay invariant under a range of conditions, to be conserved, such as energy, charge, momentum.

Thus I think that the rule I tentatively stated at the beginning: '*A good theory is a good mnemonic*' may be seen to be founded on an isomorphism between theory-construction and a certain type of memorising.

Since the code itself adds length to the message, there is an optimum coding beyond which further coding would increase the total length of the message. The '$X \equiv 0$' formulation is shown up to be extremely lengthy allowing for the statement of coding.

I have, without apology, implied an original message of finite length to express the semantic content of either theory. This may be appropriate in special cases; in most cases it is unknown whether the universe of discourse is finite or infinite, but we may imagine that we are starting with a common basis message at such a theoretical level that the message is already linguistically finite in length.

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[1] R. C. Oldfield, 'Memory Mechanisms and the Theory of Schemata', *Brit. J. of Psychology* (General Section), 1954, **45** 14

I now come to the vexed question of basis-*language*. We must, of course, prevent admission of alternative basis-languages that would result in different ordering of theories according to simplicity. It is necessary, and possibly sufficient, that the basis-language should distinguish verbally between discernibles (i.e. between situations that are distinguishable either by direct observation, or as the result of experimental investigation). Such a requirement is, I think, in any case reasonable.

The need for some restriction on the choice of basis-language is apparent from the following: We could, of course, always reduce a curve to a straight line and at the same time change a straight line into a curve by an appropriate change of co-ordinates, thus inverting the order of simplicity.

But our co-ordinates are ultimately defined operationally in physics, e.g. in classical mechanics by the translation of a rigid rod, and using the same length of rod as unit along a co-ordinate axis constitutes a preferred co-ordinate system, as long as our language is capable of distinguishing between rods of different lengths. Thus we do have scales of co-ordinates that are preferred for physical reasons, being more directly related to certain operations of measurement. Similarly there are 'natural' parameters to be used, though closer inspection will show that their choice, too, is based on some form of empirical correlation. Thus the straight line which might be defined in mechanics with reference to the motion of massive bodies, may be correlated with the straight line in optics defined by a light ray.

Simplicity frequently amounts to expressing a law in terms of the natural numbers. The preference for natural numbers *might* be considered a convention, but coding into the natural numbers has the advantage that by defining the successor operation we can simplify further by re-coding.

G. N. Lewis's claim that physical laws, expressed in his ' ultimate rational units ', are likely to involve coefficients made up of small natural numbers and multiples of π is, then, a claim that simplicity will show up linguistically on coding into a preferred system of units specified in advance. Similarly, Einstein's statement quoted at the beginning of this paper, amounts to a demand for linguistic simplicity in the covariant formulation.

Finally, there is another criterion of merit for a theory, its fertility. I shall refer to it as Simplicity III, *pragmatic simplicity*. Again, there is an interesting link between Simplicity III and Simplicity II. In this case it is not a logical relation, but the existence of a connection is a matter of experience in science (see the remark on the absence of 'blind alley' theories below).

The merit of a theory lies not only in its logical content, but in its suggestive power. We might consider the strict logical implications of a theory as its dividends. In that case good theories generally yield a bonus as well.

Thus Maxwell's theory predicted the phenomenon of displacement current as a logical implication. But Maxwell's equations also exhibited Lorentz-invariance, and were the starting point for Einstein's special theory of relativity, which substituted Lorentz- for Galileo-invariance throughout physics. This was suggested, but not implied, by Maxwell's theory.

Then there is the general experience that a theory reveals aspects of reality beyond those explicitly stated in the theory's most rigorous formulation. Molecules postulated on chemical grounds can practically be 'seen' these days: Kekulé's benzene ring model was a theory within formal chemical valency theory. But images of such rings have now been seen projected directly on to the screen of a field emission microscope. Particles postulated to explain gas pressure can now be counted. A molecule 'long' according to its structural formula exhibits mechanical properties of blocking the beaker it is in. Such extensions of experience may well exceed the expectation of the authors of the original theory.

Bridgman[1] points out that the existence of two or more independent operational definitions of a concept may be considered to justify referring to the concept as 'real'. We have referred to such 'natural parameters' under the heading of 'semantic simplicity'. Schlesinger[2] makes the further point in his exposition of Bridgman's views, that we should expect still further manifestations of such a 'real' quantity. That is one statement illustrating 'pragmatic simplicity'. Again, Maxwell's[3] distinction between physical and mathematical models brings out the suggestive power of the physical features of a model, pragmatic simplicity, as distinct from the mathematical model designed to show up formal isomorphism between different theories.

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[1] P. W. Bridgman, 'The Nature of Some of our Physical Concepts', this *Journal*, 1951, 1, 257-73

[2] G. Schlesinger, 'Bridgman's Operational Analysis', this *Journal*, 1959, 9, 306

[3] E.g. Maxwell 'On Faraday's Lines of Force', *Collected Papers*, Cambridge, 1880

The question whether a theory is ' worth-while ', rather than doubt concerning its internal consistency or agreement with experience, arises particularly in the case of ' intermediate level ' theories, such as in chemistry [1] and in the theory of solids, subjects which are now accepted to be ultimately embedded in fundamental physics. It is interesting that there seems to be hardly any instance of a theory that is simple at its own level and turns out to be a ' blind alley ' theory in the sense of not leading into the larger theory.[2]

Pragmatic simplicity is clearly related to the question of a heuristic procedure for *extending* theories. (All types of simplicity have some bearing on the construction of theories.) It is still called *simplicity* because it is to be measured by the *specificity* of the implied extension. The implication is not a strict logical one, but the theory is the better the more *specific* the implied extension is.[3]

The merit of Dirac's theory is not that it allowed *some* other particles. Its semantic simplicity lies in its relativistic invariance, and in its inclusion of spin. Its pragmatic simplicity lay in the fact that it might have been used to predict a specific particle with *unique* invariant charge and rest-mass, while *retaining* its semantic simplicity as specified above.

It would lead too far to consider the general question of heuristic procedure in constructing new theories. I do not share a popular view likening a scientist searching for an adequate theory to a machine scanning *at random* within the given field. Indeed, within a rather wide theoretical framework certain general considerations relating to uniqueness, invariance (of which symmetry is a familiar example), and continuity, are constantly applied by theoretical physicists, in constructing new theories of elementary particles. The first two heuristic principles are closely connected with maximising simplicity as defined above.

Certainly such criteria are applied *within some* theoretical framework. And Heisenberg's remark, a propos his new theory, that the universe has turned out to be not the best possible, but at any rate the simplest possible, has to be interpreted in this sense. *If* we start with a framework of relativity *and* require that certain known facts be fitted, the elementary particle masses may come out correctly as unique values in a theory that is the simplest *amongst those satisfying these requirements.* No universe containing as complex a creature as Professor Heisenberg could possibly be called the simplest possible. On the other hand, granted the diversity of phenomena to be explained, it is striking how simple the theories are that do such a good job.[1]

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[1] Lectures on organic chemistry can be a rich source of *ad-hoc* theories of valency.

[2] This point will be taken up and discussed in greater detail elsewhere.

[3] Throughout this paper, the view has been that the better (simpler) theory is more specific in *its* predictions (case (*b*)), or *requires* less specific conditions than the poorer theory (case (*a*)). The opposite view seems to be expressed by Kapp (' Ockham's Razor and the Unification of Physical Science ' this *Journal*, 1958, **8**, 265-81). His ' principle of minimum assumption ' is falsified, for instance, by the validity of universal constants. Even if these values should only be valid locally in space-time, the whole point of a scientific theory is that it does *restrict* possibilities.

To sum up: simplicity in scientific theories can be defined (Semantic Simplicity). The known universe is then found to be simple in two respects: it is, at least to some extent, describable by simple laws, and these laws contain suggestions how they may in turn be fitted into a wider context of still simpler laws (Pragmatic Simplicity). To make measurement of simplicity possible in principle, two points are made here: One, significant simplification is already possible on the purely linguistic level, and two, there is an essential connection between this Linguistic Simplicity and Semantic Simplicity. The procedure of simplification on the linguistic level is a system of coding (this is suggestive for a future discussion of heuristics and, incidentally, shows an analogy to memory processes). Starting with a basis-language preferred on operational grounds, an inverse measure of linguistic simplicity is provided by the length of the coded message stating the theory.

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[1] To give an example of applying simplicity criteria to work still developing: The wave-mechanical representation of a many-identical-particle system in first quantisation is clearly inept. The quantum-mechanical many-body problem is simpler than the classical one precisely because of the non-individuality of particles. The problem is still unsolved. Nature, however, provides us with consistently simple special instances of solutions (' shell structure '). It is likely that the explanation lies not in postulating special *ad-hoc* mixtures of interactions, but in the general analytic solution of the many-identical-particle problem.

ON PARTIAL IDENTITY OF CAUSE AND EFFECT

PAUL G. MORRISON

I

IT appears to be an essential feature of most views of efficient causation that a causative event both begins sooner than its effect does and ends after the effect begins. And from such a standpoint, it would obviously be inconsistent to regard *all* of some causative event as part of its immediate effect. On the other hand, descriptions of certain common biological processes appear susceptible to an interpretation allowing for cases in which *most* of a cause is construed as part of its effect.

The aim of this essay is to explore such an interpretation, as well as an alternative view which accommodates the same descriptive subject matter, but which rules out calling one event the cause of another immediately resulting from it whenever the two have even a single space-time part in common.[1]

An example from the physiology of animal motion will illustrate a causal process which might be construed according to either of these interpretations. On the contemporary view, the physiological cause of a response or vital action by a higher animal is the rapid breakdown of an energy-releasing phosphorus compound, such as adenosine triphosphate (ATP), which has been formed and stored earlier in the animal's muscle tissue. After the organism has been stimulated appropriately, each muscle to be used in the response is excited in such a way that some of its own store of ATP is broken down chemically, liberating energy for the contraction of the muscle. More generally, a certain chemical change inside the muscle tissue causes, or results in, its contraction. Now this latter statement deserves close attention.

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[1] From his declaration that the efficient cause is always contiguous to its effect, it appears that Hume regarded cause and effect as physically nonoverlapping events. See David Hume, *A Treatise of Human Nature*, ed. L. A. Selby-Bigge, Oxford, 1896, Book I, Part III.

More recently, the four versions of the principle of causality given by Carnap also appear implicitly to rule out cases in which any part of a causative event might be construed as part of its immediate effect. See Rudolf Carnap, *Introduction to Symbolic Logic and its Applications*, New York, 1958, p. 212.

For as will be shown presently, alternative referents for the expressions 'a certain chemical change' and '(muscular) contraction' can be selected in each instance of the process, so that if the word 'causes' is understood in one way, most of the cause will be identical with part of the effect, while if 'causes' is understood in the more customary manner, the cause will be *discrete* from (have no parts in common with) the effect. On the latter interpretation, however, the description of the process may become quite complicated in a different way to be brought out below.

2

To see more clearly what is involved in these alternate interpretations, let us first consider a model instance, B, of the physiological process in question, as represented in the accompanying diagram (Fig. 1). B is an episode of the career of a piece of muscle plus whatever foreign matter may be lodged in it. During the first two-thirds of the episode, the chemical breakdown of ATP inside it gives rise to a contractile motion of B to the right. During the remainder of the episode, the muscle relaxes, returning quickly to its original position. For the breakdown of ATP is completed, and hence the energy required to increase or maintain the contraction is no longer forthcoming.

All of these features of this greatly oversimplified model are evident in the diagram, which represents episode B as a sequence of fifteen *states* of equal duration, each indicated by a row of letters bearing a label ('B1', 'B2', . . . 'B15') showing its place in the sequence. B may also be divided exhaustively into nine non-overlapping *portions*, concurrent with one another and with the whole of B. Hence any state of B will be a composite of one state each from each of the nine portions of B. The five odd-numbered portions, represented in the diagram by the five vertical strings of the letter 'X', are pieces of muscle fibre. The four even-numbered portions are non-fibrous components of B which start out in state B1 as equal amounts of ATP, and which are successively broken down into waste and mechanical energy in B's later states. These even-numbered portions of B are represented in the diagram by four staggered inner columns, each consisting of a string of impressions of one of the first letters of the alphabet.

The breakdown of ATP in any non-fibrous portion of B is represented in the corresponding column of the diagram by the appearance of a capital letter in one row, and of the corresponding lower-case letter one row higher. Thus, in the second column of the diagram, we find a 'C' in the bottom row, and a 'c' in each higher row, indicating that in the passage from the first to the second state of episode B, the ATP in the second portion of B was broken down into waste which then persisted through the remaining thirteen states of that portion.

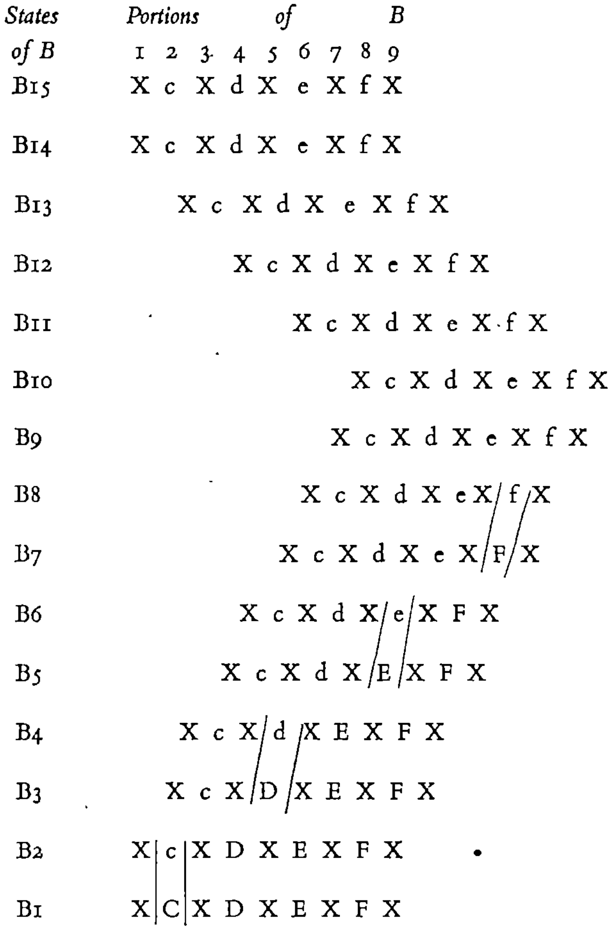

FIG. 1

The steady contractile displacement of B and of each of its portions to the right in states B2 through B10 is indicated by the offset of each of the corresponding rows in the diagram to the right of the row next below it. The relaxation of B—its rapid return to the left in states B11 through B14—is symbolised by the sharper displacement to the left of the corresponding upper rows.

3

Let us consider some causal aspects of this complex of events, speaking first of the increment of contraction resulting from the chemical breakdown of some one non-fibrous portion of B. In the passage from any one of B's first four odd-numbered states to its even-numbered successor, the ATP in the odd-numbered state of one of the non-fibrous portions of B is broken down chemically into waste in the next even-numbered state of that portion, and the mechanical energy released during the breakdown is expended in B's next two states in moving the whole of B uniformly to the right. Thus, the breakdown of the ATP in the second portion of B is represented by the appearance of a large ' C ' in the second position of the bottom row in the diagram, and the occurrence of a small ' c ' in the next higher row. The consequent motion of the whole of B to the right, resulting from the chemical breakdown Cc, is represented by showing the row standing for the state B3 one place further to the right than the row standing for B2, and the row representing B4 one place further to the right than that representing B3.

We may now distinguish more clearly the two sorts of causal interpretation mentioned above. On the first of these, the cause of the contraction is taken as the scattered individual consisting of four mutually isolated parts of B—the chemical reactions Cc, Dd, Ee, and Ff—while the contraction itself is the individual made up of the unbroken sequence of states from B2 through B10. Since it was postulated that all states of B have the same duration and that each non-fibrous portion of B began with the same amount of ATP, it follows that three-fourths of the chemical breakdown is a physical part of the resulting contraction in B. For the last three increments of chemical change—Dd, Ee, and Ff—are parts of the contractile motion, since each of them is an instance, not only of the breakdown of ATP, but also of part of a displacement to the right caused by a previous breakdown of ATP. Moreover, the model presented here has been oversimplified to the extreme. For in describing an actual instance of muscular contraction, it would be necessary to postulate a good many more than four increments of chemical change and contractile motion.

And consequently, considerably more than three-fourths (but never all) of the chemical breakdown in the muscle would coincide physically with part of the movement resulting from it.

On the second interpretation, for which causes are to be construed as physically discrete from their effects in every case, it will be necessary to look upon the chemical breakdown in B, not as a single scattered causative event, but rather as four different causes—the chemical reactions Cc, Dd, Ee, and Ff. And similarly, instead of taking the contraction in B as a single effect, we must construe it as four different effects—the increments of contraction B3-4, B5-6, B7-8, and B9-10. In short, to avoid taking the whole contractile process in B as one in which the cause overlaps the effect, we must look upon the whole process as a set of four lesser causal processes so chosen that the causative part of each is discrete from its proper effect. For in the example, chemical change Cc results in contractile motion B3-4 without overlapping it; chemical reaction Dd leads to motion B5-6 without overlap; and so on. Once again, however, in explaining any actual case of muscular contraction, we should have to divide the overall event conceptually into a great many more than four—perhaps four thousand, perhaps forty thousand—causal events in order to keep cause and effect from overlapping. Hence it would seem that the simplicity of concept inherent in construing cause and effect as occurring always in discrete places must be paid for in special cases like the one we have been considering by replacing a single causative event, x, with a myriad of lesser causes, $y1$, $y2$, . . . y_n, and dealing with the summation of the latter as a class, f, of which they are the members.

On the other hand, still another course is open to those who want to maintain a concept of non-overlapping cause and effect without being forced to consider entities like the total chemical change and the overall contraction in B as classes of their increments. For while refusing to speak of the whole chemical breakdown in B as *causing* the muscular contraction which it overlaps, and while refusing to speak of the overall contraction, conversely, as an *effect* of the chemical change, we may make a terminological decision to call the contraction a *result*, even though not an effect, of the chemical breakdown. A result, in this sense, would amount to a totality of effects—a composite event having those effects as its physical parts, rather than a class having them as its instances. And in the frequent cases in which that totality of effects does not overlap the totality of their causes, the result would also be an effect.

In fact, construing cause and effect as asymmetric converse relations between physically discrete events occurring at different times, we may define 'x results from y' in that general sense as follows: *x RESULTS from y, when there are two equinumerous classes of events, f and g, such that each event in g causes exactly one event in f, and such that x is the individual sum of f, and y is the individual sum of g.*[1] By this definition, *x* and *y* are physical individuals. For each is the individual sum of a class, just as New England is the individual sum of a certain class whose members are states in the north-eastern part of the United States. Moreover, the definition accommodates our earlier example. For no provision in it requires the overall chemical breakdown to be discrete from its result, the muscular contraction in B. And finally, the decision to distinguish result from effect would have the important consequence that every effect of an event would also be a result of it, while only those results which were discrete from the events producing them would also be effects of them.

4

As formal exploration of the conceptual framework of the biological and behavioural sciences continues, it is quite likely that a number of other causal processes commonly studied in those disciplines will be found to exhibit the kind of physical overlap spoken of here. In anticipation of that development, this paper was written mainly to show a natural pattern of events which calls for a modification, one way or another, of the conceptual apparatus now used in describing causal processes. Perhaps this modification should take a form like that of our first interpretation, involving a decision to generalise the reference of the words 'cause and effect' to cover situations with the kind of physical overlap found in episode B above. Or perhaps we should adopt an interpretation like the second one, choosing a different pair of expressions, such as 'producer and result', to carry this more general meaning, while adhering to the older use of the words 'cause and effect' to cover the very considerable subclass of cases involving no overlap of the kind found in B.

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[1] This definition is broader than our physiological example would require. For it leaves open the possibility that some members of a causally related group of events are both effects of earlier events in the group and causes of later ones. This additional scope was provided to accommodate those cases of reciprocity involved in the important phenomenon of *feedback*. For even though feedback involves an interchange of causal rôles between two individuals, it can always be described in terms of a uniform temporal process—in terms of an asymmetric causal progression from one non-recurrent state or stage of the two individual careers in question to a later one.

At present, it is not clear which of these alternatives is preferable. But in working toward a progressively more uniform general terminology for the language of science, it is often desirable to bring out general inadequacies in the language currently used to describe a given subject matter and to suggest tentatively various ways of circumventing them, without also pressing immediately for the adoption of some one alternative by everyone who deals with that subject matter.

5

Many philosophers would dismiss the views just expressed, on the grounds that causal terminology is outmoded in the language of science. They would maintain that, since the advent of the Uncertainty Principle in microphysics, causal formulations of empirical regularities have been shown to be inferior in principle to statistical ones, and that, in any case, empirical science deals only with probability connections between features of the world, while causal statements unrealistically assert necessary connections.

In answer to this view, we may admit that necessary causal connection is an outmoded concept in empirical science. On the other hand, adopting the viewpoint implicit in the cybernetic concept of a Markovian machine, we may usefully replace the older notion of necessary effect by the more general concept of probable effect.[1]

Another group of philosophers would call upon disciplines like theoretical physics for indications of the superfluity of causal generalisations in science. They would point out that the theoretical scientist has long been replacing causal statements by quantitative ones which correlate magnitudes—such as force, mass, and acceleration, for example, in the mechanical law '$f = kma$'. And since, in any specific instance of its application, 'f' 'm', and 'a' refer to values obtained through simultaneous measurements, this law is obviously not formulated in terms of efficient causation. These philosophers would argue, therefore, that the proper aim of the physiologist in the case considered above would be to specify a functional law correlating increments of chemical change with increments of contractile motion, rather than to speak in an outmoded way of a causal connection between the chemical change and the muscular contraction.

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[1] Cf. W. Ross Ashby, *An Introduction to Cybernetics*, New York, 1956, pp. 225 ff.

The difficulty with this position is that the magnitudes to be correlated in the physiological example are not simultaneous ones. For in each instance, they characterise the temporally staggered components of a common process. In each case, the release of chemical energy starts before the muscular contraction does, since it takes a short time for the released energy to pass on to the contractile proteins which are to move the muscle. Hence, unlike the above-mentioned mechanical law, any version of this physiological law which might correlate the relevant magnitudes in non-causal language would, nevertheless, be equivalent to various possible causal formulations, however cumbersome, in ordinary textbook prose.

More generally, when we turn from the equations of theoretical physics to systematic exposition in electronics, physiology, or cybernetics, we discover a fundamental concern for cause-and-effect relations in the probability sense. For investigators in the latter disciplines are looking for laws which describe repeatable patterns of change within local systems with known persistent structures. And when these repeatable processes are divided into stages or states, they find it indispensable to refer to certain features of later states as *effects* of earlier features of those processes.[1] In fact, such basic cybernetic concepts as feedback, channel, and stability appear to be inseparable from a causal concept of some kind.

To sum up, while it appears that causal terminology has lost its usefulness in the more highly formalised parts of sciences like theoretical physics, it still appears to play a vigorous and useful rôle in those parts of science which are more directly concerned with the control of process in our technical and everyday pursuits.

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[1] Cf. Ashby, op. cit.; especially his definitions of 'immediate effect' and 'ultimate effect' on pages 56 and 57.

DISCUSSIONS

MORE ABOUT LORENTZ TRANSFORMATION EQUATIONS

WHEN an author and his critic make peace it would be rash for a third person to comment, but this note is only in amplification of what Professors Grünbaum and Popper had to say in the Discussions appearing in this *Journal* ;[1] it is more about what was not said than about what was.

Popper[2] remarked that the FitzGerald-Lorentz (F.L.) contraction hypothesis mainly and merely explained the null result of the Michelson-Morley (M.M.) experiment without making additional testable predictions. Grünbaum[3] commented that the F.L. contraction hypothesis is testable in the case of the Kennedy-Thorndike (K.T.) experiment and that in this case the hypothesis makes a falsifiable prediction to the effect that a positive shift can be observed. He added that Lorentz invoked a further *ad hoc* hypothesis of time-dilatation to make the experimental evidence available at his time conform to the expectations of the aether-theory. He did not explicitly say so but seemed to imply, and rightly, that the linear contraction and time-dilatation hypotheses could explain the K.T. experiment. Popper agreed approvingly, adding that although the F.L. hypothesis was an advance in the sense that it made a testable, but an erroneous prediction, it was more *ad hoc* than special relativity and talked of degrees of ' adhocness ' related inversely to degrees of testability.

The first point we want to make is that the time-dilatation concept, although it chronologically followed the suggestion by FitzGerald of a linear contraction, was, with Larmor[4] complementary to the latter and preceded Einstein's theory of special relativity (S.R.). Lest Larmor's *Aether and Matter* should not be easily accessible, we quote Whittaker.[5]

> Larmor, *Aether and Matter* (1900), in commenting on the FitzGerald contraction had recognised that clocks as well as rods are affected by motion : a clock moving with velocity v, relative to the aether *must* run slower, in the ratio
>
> $$\sqrt{1-\frac{v^2}{c^2}} : 1 \quad .$$

The second point is that with Lorentz[6] the equations for contraction and dilatation were not just *ad hoc* assumptions to explain only this or that experiment but followed from the equations for transforming electrodynamic equations from a frame at rest relative to the aether to another frame in motion relative to the first. We are not maintaining that Lorentz's transformation equations of 1903 meant the same thing as Einstein's[1] of 1905, although both have exactly the same algebraic form. Lorentz indeed, assumed an absolute time and an absolute standard of length associated with a frame at rest in the aether. At a conference[2] on the M.M. experiment held at Mt. Wilson on the 4th and 5th February 1927, where Michelson, Miller and Kennedy were present, Lorentz said :

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[1] This *Journal*, 1959, 10, 48-50

[2] K. R. Popper, *The Logic of Scientific Discovery*, 1958, p. 83

[3] There is a minor mis-statement in Grünbaum's comment which refers to the horizontal and *vertical* arms of the interferometer in the M.M. experiment, etc. In fact, both the arms lay in a horizontal plane.

[4] Larmor, *Aether and Matter*, Cambridge, 1900; see, for example, p. 174

[5] E. T. Whittaker, *History of the Theories of Aether and Electricity*, II, p. 32 n.

[6] Lorentz, *Proc. Amst. Acad.* (English edition), 1903, 6, 809. See also p. 31, Whittaker, op. cit.

> A transformation of the time was also necessary. So, I introduced the conception of local time which is different for different systems of reference which are in motion relative to each other. But I never thought that this had anything to do with real time. This real time for me was still represented by the older classical notion of an absolute time, which is independent of any reference to special frames of co-ordinates. There existed for me only one true time. I considered my time-transformation only as a heuristic working hypothesis. *So, the theory of relativity is really solely Einstein's work.*[3]
>
> *Asked if I consider this contraction as a real one, I should answer ' yes '. It is as real as anything we can observe.*

Thirdly, it appears to us that L.T.E. in the context they were derived by Einstein lead to logical difficulties. In the present note we shall deal with one such difficulty. But the same equations according to Lorentz's ideas can be consistently interpreted and applied. This is possible because the physical bases in the two cases are different. It is, of course, true that the implications of L.T.E. were not fully worked out by Lorentz before Einstein. In particular, the extension of L.T.E. to all physical phenomena and not merely to electrodynamics is due to Einstein. But even Einstein's special theory took years for development, and all the predictions and consequences of the theory were, and could not have been worked out, in the first paper of Einstein (1905). But the transformation equations with Lorentz as well as Einstein are the same algebraically.

Let us first quickly derive L.T.E. according to the theory of relativity. This is necessary to make sure that the arguments can be developed internally in this note to the extent it is possible to do so. We use a method commonly employed.

S′ is a frame of reference moving with velocity v relative to frame S in the direction of x and x' axes. The two frames are equivalent for the description of the laws of nature, in particular the velocity of light in all the directions is c in both the frames. Consider a light signal originating at the instant the origins of the systems S and S′ coincide. The spherical front of the expanding light-wave in the two frames is given by $x^2 + y^2 + z^2 = c^2t^2$ and $x'^2 + y'^2 + z'^2 = c^2t'^2$. Assuming $y' = y$ and $z' = z$, we have

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[1] Einstein, *Electrodynamics of Moving Bodies*, *The Principle of Relativity*, New York, 1923

[2] *Astrophys. Journal*, 1928, **68**, 385-388

[3] At the Jubilee of Relativity Theory held in Berne in July 1955, Max Born in his address entitled ' Physics and Relativity ', provoked by Whittaker's opinion that the special theory originated with Poincaré and Lorentz, laboured the point regarding priority in the discovery of relativity theory, but he had apparently not come across this statement of Lorentz. A remark of Lorentz to the effect ' To discuss Einstein's Principle of Relativity here, etc.' quoted by Born is weaker in assertion than the statement of Lorentz quoted above.

$$x^2 - c^2t^2 = x'^2 - c^2t'^2 = 0.$$

We see by substitution that the following transformation equations hold good :

$$x' = x \cos\phi + ict \sin\phi \qquad i = \sqrt{-1}$$

$$ict' = ict \cos\phi - x \sin\phi,$$

in which ϕ has to be identified.

Where $x' = 0$, $x = -\,ict \tan\phi$. Now the origin O′ of S′ ($x' = 0$) will have in S a variable co-ordinate $x = vt$. We *may*,[1] therefore, put $vt = -\,ict \tan\phi$, thus giving

$$x' = (x - vt)\Big/\sqrt{1 - \frac{v^2}{c^2}},\ y = y',\ z = z',$$

and

$$t' = (t - vx)\Big/\sqrt{1 - \frac{v^2}{c^2}}.$$

These are L.T.E. Although they were derived only for an optical case, they were *assumed* by Einstein to be applicable to all physical phenomena. We shall now apply them to transform, a sphere of light-wave propagated in the frame S, from this frame S into the frame S′ ; in particular, we shall find the *space geometry* (x', y', z') of the wave-front in S′ at time t'.

In S, the intercepts on the three axes are :

$$x = r,\ y = r \text{ and } z = r,$$

at time $t = r/c$.

Transformed to S′ the corresponding observed intercepts on the three axes are :

$$x' = (x - vt)\Big/\sqrt{1 - \frac{v^2}{c^2}} = (r - vr/c)\Big/\sqrt{1 - \frac{v^2}{c^2}} = r\left(\frac{c - v}{c + v}\right)^{1/2}$$

$$y' = r \text{ and } z' = r$$

at time $t' = \left(t - \dfrac{vx}{c^2}\right)\Big/\sqrt{1 - \dfrac{v^2}{c^2}} = \left(\dfrac{r}{c} - \dfrac{vr}{c^2}\right)\Big/\sqrt{1 - \dfrac{v^2}{c^2}} = \dfrac{r}{c}\left(\dfrac{c - v}{c + v}\right)^{1/2}.$

The expanding sphere (r, r, r) in S is thus transformed into an ellipsoid $\left[r\left(\dfrac{c - v}{c + v}\right)^{1/2}, r, r\right]$ in S′.

---

[1] There are infinitely many relations which satisfy the equations

$$x^2 - c^2t^2 = x'^2 - c^2t'^2 = 0.$$

Even the number of linear solutions is infinite, e.g.

$$x' = \Big[x - \psi(\mu)\, t\Big]\Big/\left[1 - \frac{\psi^2(\mu)}{c^2}\right]^{\frac{1}{2}}$$

$$t' = \left[t - \frac{\psi(\mu)\, x}{c^2}\right]\Big/\left[1 - \frac{\psi^2(\mu)}{c^2}\right]^{\frac{1}{2}}$$

whatever μ may be physically, $\psi(\mu)$ being any function of μ whatsever.

The simple calculations given above also show that the velocity of propagation along the y' and z' axes is not c but $c\left(\dfrac{c+v}{c-v}\right)^{1/2}$.

Has there been a mistake? We have used L.T.E. for transforming from the frame S to the frame S′ and that is what the transformation equations are for. Einstein, in his first paper of 1905, while dealing with the transformation of light-rays (not the shape of material bodies) also said :[1]

> The spherical surface—viewed in the moving system—is an ellipsoidal surface. . . .

But a little earlier in the same paper (p. 46) he stated:

> The wave under consideration is therefore no less a spherical wave with velocity of propagation c when viewed in the moving system. . . .

The principle of relativity demands that a spherical wave-front of light be observed as spherical in the 'moving' system, but L.T.E. do transform it into an ellipsoid because L.T.E. leave they y and z co-ordinates unaffected but not the x co-ordinate. The time co-ordinate is transformed homogeneously (i.e. equally for all the three space co-ordinates) giving a velocity of propagation along the y' and z' axes greater than c.

L.T.E. no doubt transform $x^2+y^2+z^2-c^2t^2$ into $x'^2+y'^2+z'^2-c^2t'^2$, but when we consider the transformations *separately for space and time co-ordinates*, we notice that the two frames S and S′ are *not* equivalent. When $x^2+y^2+z^2-c^2t^2$ is transformed by L.T.E. into itself, v, the relative velocity disappears, but v enters into the transformation equations of space and time co-ordinates taken separately. Now, in any experiment, space and time measurements usually are, or, at any rate, can legitimately be made separately and the 'equivalence' of two systems in a 4-dimensional continuum is hardly enough to secure 'equality' in all measurable physical respects.

There is thus a basic antagonism between L.T.E., which discriminate between two frames in relative motion, and the principle of relativity which does not. The notorious clock-paradox also arises from this antagonism.

Various subterfuges are employed to explain L.T.E. consistently with the principle of relativity. The contraction, etc., are called 'apparent', or it is said that there is only one observer in frame S etc., etc. It will be surely noticed, however, that (x, y, z, t) and (x', y', z', t') are the co-ordinates *assigned and measured in the two frames without stating a point of preference for one or the other and without any specification regarding the observer or the mode of observation.*

It is often asserted that the transformations are reflexively true, and that this fact establishes the equivalence of the two frames—a reflexivity which is established merely by changing signs and superscripts. The question, physically, is simply this: when we go from one inertial frame to another in uniform motion relative to the first, how do we transform the co-ordinates from the first to the second? In particular, do the contraction and dilatation of S.R. obtain? Three answers are usually given: the question is meaningless; yes, the contraction, etc. exist, but they cannot be detected; no, because all inertial frames are equivalent, or, since the standard of rest can be arbitrarily chosen and contraction, etc. can thus be arbitrarily ordered, they do not occur. Since the first view-point is less known, we are quoting Sommerfeld,[1] one of its proponents.

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[1] Einstein, *Electrodynamics of Moving Bodies, The Principle of Relativity*, New York, 1923, p. 57

> The frequently raised question whether the Lorentz contraction is 'real' or 'apparent' is, of course, just as idle as the question whether a body 'actually' moves. The distinction between a moving system and a system at rest, which we permitted in the preceding (development of L.T.E., etc.) for the sake of simplicity of expression, is equally meaningless and arbitrary.

Before we deal with the interpretation of experimental evidence, we mention a formulation of the principle of relativity in terms of what Whittaker[2] called a postulate of impotence, to the effect: 'It is impossible to detect a uniform translatory motion, which is possessed by a system as a whole, by observations of phenomena taking place wholly within the system.' In the explanation of various experiments to determine the velocity of the earth by terrestrial observations, sometimes this postulate and sometimes L.T.E. are invoked but, generally, the postulate alone is quoted to explain the experiments, L.T.E. being distrusted somewhat, because a few of the experiments cannot be explained by L.T.E. For example, in the experiment of Wood, Tomlinson, and Essen,[3] on the change in the frequency of a rod rotated horizontally and thus subjected to different magnitudes of F.L. contraction which yielded a null result, the complete compensation of the longitudinal contraction by a change in the elasticity cannot be explained by L.T.E.

It might be said at this stage that it is precisely for such reasons that S.R. is superior to a scheme in which L.T.E. are based on a fixed aether. But wait. L.T.E., according to S.R., admittedly, should be competent to deal with cases of relative motion or one in which a change in the inertial frame occurs. Now when an apparatus is rotated or is carried by the earth from one position along its orbit to another, there is undoubtedly a change in the inertial frame. In the case of rotation of an instrument through a right angle, for example, the x and y axes exchange their positions and there are different transformation equations for the two axes. Astronomical aberration can be satisfactorily explained by S.R.[4] only on the basis of a change in the inertial frame of the earth as it courses along its orbit. Why is it, therefore, that the *changes* in the motion of the earth in its orbit cannot be detected by terrestrial observations? L.T.E. indicate that this should be possible. After all, L.T.E. are successfully applied to all experiments in which relative motion occurs on the earth itself—for example, in the experiment of Ives and Stilwell.

According to Lorentz's ideas, we can approach the results of various experiments frankly. The first-order effects such as aberration, Fizeau's water-tube experiment and Doppler-shift are explained with the help of L.T.E. A few of the second-order, non-optical (or partly optical) experiments, such as those of Ives and Stilwell, and of Otting and increase in the life of fast mesons are likewise satisfactorily explained. The purely optical second-order experiments such as those of Michelson and Morley, and of Kennedy and Thorndike, are, of course, adequately explained by L.T.E. and, contrary to the belief of some writers, the null results of the experiments of Rayleigh and of Brace on double refraction in matter passing through the aether, of Trouton and Rankine on the change in the electrical resistance of a conductor differently oriented relative to the direction of motion of the earth, and of Trouton and Noble on the mechanical forces acting on a charged electric condenser moving through the aether, can all be satisfactorily explained in terms of Lorentz's electrical theories (parts that are still valid and accepted) and L.T.E.[1] There are, however, a few experiments, such as Wood's quoted earlier, which *cannot* be explained by L.T.E.[2]

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[1] Sommerfeld, *Electrodynamics*, New York, 1952, p. 227

[2] Whittaker, *From Euclid to Eddington*, Cambridge, 1949, p. 58

[3] Wood, Tomlinson and Essen, *Proc. Roy. Soc.* A, 1937, **158**, p. 606

[4] Leigh Page, 'On the Aberration of Light', *Astrophys. Journal.*, 1925, **61**, 70

It is interesting to note that, although time-dilatation has been observed directly in the experiments of Ives, Stilwell, and Otting, no direct observation of F.L. contraction of length has yet been made.[3] To be exact, only a change in frequency—and as measured in the '*other*' system (source in S′ and observer in S)—was recorded in these experiments. It is, however, assumed that the change in frequency arises from the 'slowing' of atomic processes, and that the frequency in S′ would also be the same were it observed in S′ because the classical Doppler effect is separated in these experiments from the time-dilatation effect. In the case of fast mesons, possibly, time-dilatation is directly observed.

To summarise, although the principle of relativity asserts that the laws of physics have the same form in all inertial systems, an examination of space and time transformations *separately* shows that the inertial system assumed as the standard of rest, in deriving L.T.E., *is* a preferential system. The aether qualifies for this preferential system. Although the aether has not been experimentally discovered, only its postulation makes a logical interpretation and application of L.T.E. possible.

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The Principle of Minimum Assumption

Professor R. O. Kapp's suggested principle of minimum assumption[4] is most alluring. Not only is it simple and apparently universal in application, it is also claimed to yield a great variety of concrete results. But no matter how much initial sympathy we may have with Kapp's thesis, the unfriendly realities must be faced. The hard facts are that the discovery of principles of such fundamental character and with such far-reaching predictive and explanatory power is not as easily brought about as all that.

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[1] Sir James Jeans, *The Mathematical Theory of Electricity and Magnetism*, Cambridge, 1951, pp. 595-596

[2] Cullwick believes that M. Wilson and H. A. Wilson's experiment on a rotating magnetic insulator (*Proc. Roy. Soc.* A, 1913, 89, pp. 99-106) discriminates between the two theories verifying only the formula based on Einstein's ideas. We shall comment on this separately. See pp. 161-174 of his book, *Electromagnetism and Relativity*, London, 1957.

[3] It is not necessary for a system to be physically discovered, to sustain a physical theory. Witness various quantum theories including the postulation of neutrino. This ubiquitous particle may indeed have something to do with the aether.

[4] R. O. Kapp, 'Ockham's Razor and the Unification of Physical Science', this *Journal*, 1959, 8, 265-280

Here I wish to raise some of the difficulties that become apparent upon a closer inspection of Kapp's paper, and mention them in the order of increasing perplexity. It seems an inevitable conclusion that the principle, as it stands, is unacceptable. Whether or not, in some modified and more restricted form it may yet be entertained, is a matter for much careful analysis.

Kapp makes it clear at the outset that his aim is a very radical one. He intends to show that Ockham's Razor (or rather his interpretation of it) is no mere methodological rule advising scientists to adopt provisionally a procedure which may or may not lead to the correct result. The principle is a direct outcome of the objective character of the laws of nature. Thus Kapp announces:

> I wish to raise it [Ockham's Razor] from a mere rule of procedure to one of the great universal principles to which the whole of the physical world conforms. At this level it would be worded as follows: *In physics the minimum assumption always constitutes the true generalisation.*[1]

Further on [2] he defines a minimum assumption ' as one that is completely unspecific '. And this he explains by means of examples. Finally he says: ' *In physics a generalisation that is logically possible is also physically possible.*' [3]

I propose to examine in some detail only two of the numerous concrete results that allegedly follow from the above principle. One concerns the claim that the falsity of the doctrine of ' horror vacui ' follows directly from the rule of minimum assumption Kapp says:

> . . . it was assumed at one time that Nature has specific likes and dislikes; for instance, that she abhors a vacuum. This could be translated into contemporary language as the specific assumption that a law of physics prevents the density of matter from falling below a specific value. But it is now known that the true generalisation about the density of matter is unspecific. The laws of physics permit any density, ranging from the high concentration that occurs in the white dwarf stars to the extreme tenuousness of extragalactic space.[4]

The second example refers to a more recent scientific result, namely to the discovery of Hafnium, the element with seventy two unit charges in its nucleus. This element remained unobserved long after the construction of the periodic table, but its existence was predicted and according to Kapp it ' was predicted purely on the basis of this principle '.[5] For, he explains, the principle forbidding specific assumptions demands that there be nuclei with *any* number of charges, including the number seventy two. He goes even further and points out that it should have been clear that *any* includes zero as well. Consequently the existence of the Neutron, too, should have been predicted on the basis of the principle before it was actually observed.

An immediate question that arises in connection with the last example is: how do we account for the fact that there are no nuclei containing more than ninety-two charges? Clearly the unspecific assumption implies that there be no upper limit to the number of charges a nucleus may have. This question is answered by Kapp himself. It is answered, however, not by insisting that the present state of affairs is merely a temporary one, and that eventually nuclei with any number of charges are going to be observed, but in the following manner:

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[1] R. O. Kapp, 'Ockham's Razor and the Unification of Physical Science', this *Journal*, 1959, 8, p. 272

[2] Ibid., p. 273

[3] Ibid., p. 279

[4] Ibid., p. 27

[5] Ibid., p. 276

> For the elements to be found in nature the maximum number of such charges is ninety-two, a stability limit. This limit provided a logical reason why no nucleus could be observed in nature with more than ninety-two unit charges, for a greater number would be inconsistent with the definition of a stable particle.[1]

These last words are somewhat puzzling. True enough, to call a nucleus stable when we can see it disintegrate in front of our very eyes ' would be inconsistent with the definition of a stable particle '. But there is nothing contrary to logic in insisting that a nucleus of ninety-eight or more charges does not disintegrate and therefore is to be found in a stable state. It is only inconsistent with observation.

It seems therefore that the only way Kapp could assign a ' logical reason ' for the disintegration of such nuclei is by claiming that it follows logically from the laws of forces which apply within a nucleus, that they cannot remain stable. From this we may then infer the sense in which the expression ' logically possible ' is employed in the statement ' In physics a generalisation that is logically possible is also physically possible '. The expression appears to have been used with the intention of excluding not only generalisations which are self-contradictory but also generalisations that stand in contradiction to other established propositions. In our case then, the generalisation, that the number of charges constituting the nucleus may be as great as you like, is to be regarded as ' logically impossible ', as it contradicts the propositions embodying the laws of forces governing the interior of nuclei.

This last idea is of central significance. It would have to be used by Kapp in reply to many questions one might ask against his principle. For example: if the principle is valid, why is there a lower limit below which the temperature of a material system does not drop? The completely unspecific assumption is surely that the temperature of a body may be anything. How do we then account for the existence of the absolute zero of temperature? Or in the same manner we can ask: Why can a body not acquire a velocity greater than that of light, when according to the principle any velocity should be possible? Kapp does not pose these questions but one can imagine that his answer here, too, would be that propositions stating the existence of an absolute zero of temperature or the existence of an upper limit to the magnitude of velocity attainable are not independent; they follow from other given propositions. In the former case the given propositions are those constituting the kinetic theory of heat, and in the latter the special theory of relativity.

But here arises a very serious difficulty with Kapp's thesis. Is it really clear in all cases where we are confronted with two sets of propositions, which is to be regarded as ' given ' and which as ' follows '? In other words, since the principle of minimum assumption only applies to ' primary ' propositions which *entail* others but not to ' secondary ' ones *entailed* by others, we must have some unequivocal method whereby to distinguish between the two, before the principle can be put to any use. But we were not given any such method.

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[1] R. O. Kapp, ' Ockham's Razor and the Unification of Physical Science ', this *Journal*, 1959, 8, p. 276

Kapp's claim (quoted above) about the doctrine that nature abhors a vacuum is a case in point. He says that the doctrine could have been exposed as fallacious on the basis of his principle which demands that matter may exist in *any* density. But Aristotle, the protagonist of the doctrine, did not conceive it as an independent primary dogma. He presents it as a conclusion drawn from self-evident premises. For Aristotle reasons from the impossibility of infinite velocity that a vacuum is impossible. Thus, even if Aristotle were acquainted with Kapp's principle, he would not have made a generalisation permitting the existence of a vacuum, for to him that would have amounted to a logically impossible generalisation. How can we expect Aristotle to have known that his doctrine 'horror vacui' was not a secondary thesis, for it is not entailed by the true basic laws of dynamics as we know them now?

Or what is the answer to the even more difficult question, of how we really know in all the cases where we find the principle is violated, that we are dealing with secondary propositions? How do we know, for instance, that we are not to start first with the proposition that the velocity of a body may reach any degree, as demanded by the principle, take this as our primary proposition, and consequently modify all the other related proposition accordingly (even if it does mean the overthrow of the theory of relativity)? Or in the case of the periodic table of the elements, why not start by assuming, in accordance with the principle, that nuclei with any number of charges do exist and conclude that the laws of forces operating within the nucleus must be such as to allow the increase of charges to any desired value? Surely to say the laws of forces cannot be such, for we see in fact the disintegration of the heavier elements is merely begging the question.

Perhaps the above difficulty is not necessarily insurmountable. However, it certainly cannot be overlooked either. It cannot be taken for granted without adequate analysis that there is an objective and not merely an arbitrarily adjustable difference between primary propositions (to which the principle is to be applied) and secondary propositions (which are logically pre-determined and may therefore contradict the principle).

2

A difficulty of another kind is that of identifying a minimum assumption. When presented with alternative hypotheses, is it always quite clear which one contains the minimum assumption? True enough, Kapp is very reassuring on this point. He says:

> . . . one may learn the operative word by which to recognise a minimum assumption. It is 'any'. It need not apply only to numbers but can also apply to quantities, properties, relationships, configurations, to any feature that one likes to mention. When there are only two alternative possibilities, the grammatical substitute for 'any' is 'either' as, for instance, when there is a choice between the positive and negative sign. So a minimum assumption can be recognised by the use in its formulation of the words 'any' or 'either'.
>
> In practice it is not difficult to distinguish between a minimum assumption as just defined, and one that is not a minimum one.[1]

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[1] R. O. Kapp, 'Ockham's Razor and the Unification of Physical Science', this *Journal*, 1959, **8**, p. 274

But surely matters are not as simple as this. The same idea can often be formulated in different ways. The wording alone cannot be a sufficient guide in the selection of the correct hypothesis. For if it were only the word 'any' that mattered, I cannot see what prevents us from asserting 'matter can be found *any*where'—thus entailing the impossibility of a vacuum, or even 'any gap may exist in the periodic table of elements'—thus destroying the basis for the prediction of Hafnium.

3

I believe, however, that the main trouble with the principle is that Kapp has not laid down clearly the constraining conditions under which it is to operate. Just as in mathematics a minimum principle is useless without constraining conditions, so Kapp's principle, as it stands, is indeterminate. And this can best be demonstrated by showing how it admits all sorts of absurd questions.

Why, for example, does the periodic table not contain elements with fractional charges in the nucleus? We are told that by the use of the word 'any' we were made aware of the existence of Hafnium (72 charges) and the Neutron (0 charges), why not also nuclei with half or quarter of a unit charge? The answer that electric charges exist in discrete amounts only raises the more embarrassing question: why indeed? According to Kapp's principle we are to expect to find electric charges in *any* amount. Radiation too, of course, should be emitted in *any* quanta.

Similarly, why are there not *any* kinds of masses—masses which attract or repel each other according to *any* laws of distance? And if it be answered that even Newton's Law of Gravitation is not a primary law, then what is? The Law of Conservation of Energy? No, because it contradicts Kapp's Principle. For the minimum assumption is surely that all kinds of closed isolated systems should exist. There should be closed isolated systems in which the energy either increases or decreases and not only such systems in which the energy always remains the same.

It is evident, therefore, that the first and most essential step towards clarifying Kapp's principle is to give a clear statement of the constraining conditions under which it is to apply. In its present form the principle of minimum assumption is a principle of no assumption—leading only to an assertion of the type 'anything may happen anywhere, at any time'. Or more simply 'in physics anything goes'.

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REPLY TO CRITICISMS BY G. SCHLESINGER

THERE are, perhaps, some questions in physical science that cannot be so worded as to provide only two possible answers; yes or no. But most questions can be so worded provided sufficient trouble be taken with their formulation. They then sometimes appear with uncompromising clarity and prove to be such that each of the two possible answers is unattractive. Whichever answer has the support of facts and logic, it will then be deprecated by those who forget that the alternative answer would earn their disapproval too. A natural reaction to such disconcerting questions is to say that the subject is not as simple as all that, or that the answer found could only be entertained in some modified or restricted form, or that the question is not a real one, or to try to find an answer yes that is not quite yes, an answer no that is not quite no. Such efforts are sterile. They only blunt the sharp edge of the question. They offer a comforting confusion as an escape from the stern demand for a choice between unwelcome alternatives.

These remarks are relevant to Mr Schlesinger's note. I shall begin my comment on it by rewording a distinction that is to be found implicitly in the paper by myself to which he refers. The rewording may help to reveal that the choice is inescapable.

Let all general statements in physics be placed in one of two classes, Class A and Class B. Such statements may be of any kind; they may be about basic principles, about laws, about quantities, about cosmic constants; it is only required that they have universal validity.

Those to be placed in Class A are defined as the statements that can be inferred from other statements. This may be because they are tautologies, inferable from the words used for their formulation. It may also be because they are implicit in more general statements, and inferable from these. Thus the laws of planetary motion belong to Class A because they are implicit in the more general laws of mechanics. Statements in this Class are called ' reducible '. They are said to be ' explained ' by the more general statements in which they are implicit. Hence they could become known without the aid of observation and by pure inferential reasoning provided this were comprehensive and acute enough and the general statements in which they are implicit are known.

Statements in Class B are not tautologies and are not implicit in any others. They are called ' irreducible '. They cannot be ' explained ' in the sense in which a statement in physics is said to be explained. They cannot be arrived at by inference or tested by deductive processes. They are only known by observation and their validity can only be confirmed by observation. Thus the laws of planetary motion appeared in Kepler's day to belong to Class B. Kepler found them by observation and his contemporaries were in a position to confirm them by observation. Much later Newton showed that these laws could be known by inference to a person who spent his life under a blanket of cloud and had no means of observing planets. He could deduce the elliptical orbit of planets from more general principles. Therewith the laws of planetary motion were transferred from Class B to Class A. I have described Class B statements metaphorically as clauses in a Cosmic Statute Book. It is clauses of this statute book kind, and of this kind only, that ensure order in human affairs.

In the history of physical science more and more statements, once thought to belong to Class B, have been found really to belong to Class A. This has been expressed by saying that more and more observed facts have been ' explained '. The explanation has always been in terms of more general statements, like Newton's explanation of the elliptical orbits of planets in terms of the more general laws of mechanics. One cannot yet show that *all* statements in physics belong to Class A. Some are in doubt. Among them are certain very general principles and a handful of cosmic constants: the velocity of light, the quantum of action, unit electric charge, and a few others. They are known by observation only. It has not yet been found possible to prove that anyone who did not make, or know of, the necessary measurements could nevertheless infer the precise value of these constants from general principles, as he could infer the elliptical paths of planets from more general principles.

The uncompromising, the disconcerting, the unwelcome question is this: Can the process of transferring statements from Class B to Class A continue until none are left in Class B? Do all the statements that have any place in physics really belong to Class A, even when they seem at present to belong to Class B? Are all statements in physics reducible? Can every one of them be explained in the same way as the elliptical orbits of planets have been explained? Is there, in physics, nothing to which the metaphor of a Cosmic Statute Book applies? Are the laws of physics of a different kind from those laws that demand certain things and prohibit others? Is there no principle in physics to make for specified order in the sense in which the laws of a country make for specified order?

The above questions are but different forms of one single question. What is the answer: yes or no? As I showed in my paper the answer ' yes ' leads to the Principle of Minimum Assumption; the answer ' no ' is a denial of this principle.

If one thinks only of some of the implications of this question one is certainly inclined to answer ' yes '. If one thinks only of other implications one is inclined to answer ' no '.

If the correct answer is ' yes ' one may hope some day to explain why such cosmic constants as the velocity of light, unit charge, the quantum of action have precise values; why there is a principle of conservation energy; to answer sundry questions beginning with ' why ' to which no answer is yet forthcoming. If the correct answer is ' no ' some at least of these facts are intrinsically irreducible; they will never be explained. The reason why they are observed can only be that an arbitrary requirement, a clause in the metaphorical Cosmic Statute Book, leaves no alternative. When one remembers this one likes the answer ' yes '. It is a hopeful answer.

If yes is the correct answer and if there is no law or other fact of physics to which the metaphor of a statute book applies, there can be nothing in the physicist's universe of discourse by virtue of which order is imposed on the material universe. Provided only that the principles of logic be met, physics must then be a science in which, to use Mr Schlesinger's term, ' anything goes '. If it is so the physicist who hopes, by observation and experiment, to discover the laws of nature, to transcribe the requirements of the metaphorical Cosmic Statute Book into the textbooks on physics, is not pursuing the only course towards understanding of his subject. Pure inference would provide an alternative course. While the physicist is believing it to be his duty to find out what is prohibited by the laws of physics he could, instead, seek to find out what is prohibited by the principles of logic; both quests would lead him to the same conclusion. Many do not like to think that it is so. When one remembers this one is likely to follow Mr Schlesinger and hope that the correct answer is ' no ', to deprecate with him the notion that, in physics, ' anything goes '.

There is the dilemma. Human nature being what it is there is a temptation to dodge so irksome a question. But I am trying very hard so to present the problems that the question can no longer be dodged.

It is against this background that Mr Schlesinger's note has to be considered. I analyse his various points below:

(1) 'The answer found by myself could only be entertained in some modified or restricted form.' This is to express the hope that one may be able to find an answer 'yes' that is not exactly yes.

(2) 'The distinction between what is given and what follows, between a primary and a secondary proposition, is not clear.' It is admittedly difficult to find a good criterion for this distinction and so to word it that it is absolutely mistake proof. But such difficulty is no argument against seeking a criterion. Without a criterion that can withstand rigorous philosophical criticism one is still able, in practice, to distinguish quite easily between a broad generalisation and a more restricted one. Would anyone deny that the laws of mechanics are more general than the law requiring planets to have elliptical orbits? It can be shown that the law governing the maximum thermal efficiency obtainable from a given steam engine follows from the principle of least action. No-one could argue rationally that this very broad principle follows from the performance of steam engines.

(3) 'By the Principle of Minimum Assumption the periodic table could contain elements with fractional charges on the nucleus.' Admittedly the fact that the elements do not contain fractional charges could be a refutation of the Principle of Minimum Assumption. That the 'charges' are always whole numbers would then be a Class B statement. It is also true that, at the present state of knowledge, the quantised nature of electric charge has not yet been 'explained'; it has not been inferred from anything more general; it appears as an irreducible fact; it is one of the few remaining statements in physics that might have to be called a clause in the metaphorical Cosmic Statute Book. But I doubt whether many physicists would agree with Mr Schlesinger that it must always be so. The search for a unifying principle that will 'explain' the precise value of unit charge has not yet been abandoned.

(4) 'An inference from the Principle of Minimum Assumption would be that matter can be found anywhere and this would entail the impossibility of a vacuum.' No. It would only be so if one word were as good as another; if 'can' were a synonym for 'must'; if 'anywhere' were a synonym for 'everywhere'. The statement that matter *must* be found *everywhere* would entail the impossibility of a vacuum, but it would not conform to the Principle of Minimum Assumption.

(5) 'An inference from the Principle of Minimum Assumption would be that any gap may exist in the periodic table of elements.' This is a more subtle lapse from logic. The error is to assign to this statement the same status as to the statement that any atomic nucleus may carry any number of unit charges up to the stability limit. The first statement is about a man-made device for recording and arranging facts, namely the periodic table. It is not a statement about physical realities. The second statement is about physical realities, namely atomic nuclei. The Principle of Minimum Assumption applies only to this latter kind of statement.

REGINALD O. KAPP

SOME COMMENTS ON 'IMAGINARY EXPERIMENTS'

IN appendix* xi of *The Logic of Scientific Discovery* Professor Popper undertakes a critical analysis of imaginary experiments, mostly pertaining to quantum theory. With regard to Heisenberg's γ-ray microscope experiment which has become the classical illustration of the principle of indeterminacy, Popper mentions several points which in his opinion Heisenberg tried to establish, among them '*the disturbance* of the measured object by the process of measurement, *whether of position or of momentum*' (p. 451). Popper arrives at the conclusion that Heisenberg's experiment is inadequate as a basis for interpreting the indeterminacy relations, for 'it fails to establish that measurements of position and momentum are symmetrical' as they should be when conforming to the symmetrical structure of the relation $\Delta x \,.\, \Delta p \sim h$. The asymmetry referred to consists, according to Popper, in the following: In determining the electron's position one has to use light of short wavelength which leads to a *disturbance* of the electron's *momentum*. If, on the other hand, one wants to determine the momentum, one must use long wavelength radiation, and the resulting observation will *fail to disclose* the electron's position which will thus remain indeterminate not as the result of a disturbance.

Popper's argument is invalid because it is based on the erroneous assumption that it is the *disturbance* of either the position or the momentum which is the essential feature of the mechanism of interaction between the object of observation and the agency of measurement. A thorough analysis of the problem in question, amplifying on Heisenberg's explanation, has been given more than thirty years ago by Niels Bohr.[1] The main points may be presented as follows: One has to use different experimental arrangements depending on whether one wants to measure *either* the electron's position *or* its momentum—a microscope in the first case and a spectroscope in the second, and in both of them one has to take due regard of the Compton effect as well as of the limitations inherent in the means of observation.

In the case of the microscope (determination of position) the uncertainties of position and momentum involved are respectively $\Delta x = \lambda/\sin\alpha$ and $\Delta p = h\sin\alpha/\lambda$, where $\sin\alpha$ is the numerical aperture of the microscope. It is thus the finite aperture of the microscope (and not the 'disturbing' wavelength of the illuminating light alone) which is responsible in a reciprocal manner for both uncertainties. On the other hand, in determining the electron's momentum with the spectroscope, one has to measure the wavelength of the scattered radiation, and if this is done in the direction opposite to the incident light, the uncertainties of position and momentum involved are respectively $\Delta x = 2hl/\lambda mc$ and $\Delta p = \lambda mc/2l$, where l is the length of the wave train used in the observation. The shorter the latter compared with the wavelength used, the greater the inaccuracy $\Delta\lambda$ of the determination of the wavelength, and therefore of the momentum (in fact, $\Delta\lambda = \lambda^2/l$). It is thus the finite extent of the wave train scattered by the moving electron and analysed by the spectroscope, which is responsible in a reciprocal manner for the uncertainties both of momentum and position. A complete symmetry of the uncertainties is thus prevailing in conformity with the formula, if one takes into account *all the details* of the respective experimental arrangements.

It is interesting to note that, in the case of the latter experiment, the accuracy of the determination of the momentum is the higher, the *shorter* the wavelength λ. This, however, implies a *greater* (controlled) disturbance of the momentum h/λ of the observed particle. Contrary to popular belief, therefore, it is not the magnitude of the disturbance itself which determines the inaccuracy of the measurement.

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[1] *Nature*, 1928, **121**, 580; cf. especially 582–583

In another passage of the same appendix, Professor Popper warns against what he terms the 'apologetic use of imaginary experiments'. The warning is well worth to be taken to heart; it is only regrettable that the example chosen (p. 447) is not a suitable illustration. Popper quotes Einstein's imaginary experiment, devised to demonstrate the possibility of measuring simultaneously time and energy of a system with as great an accuracy as desired. The gist of the experiment is that a box from which a photon is released at a given time, is being weighed before and after the release, whereby use is made of the formula $E = mc^2$. Bohr refutes Einstein by using arguments from general relativity (change of the rate of a clock when displaced in the direction of the gravitational field). Popper objects to Bohr's argument, because of the 'extravagant assumptions' made by him in introducing general relativity in order to discard an experiment based on special relativity. But this objection is unfounded, for although the relation $E = mc^2$ is derived from special relativity, it is Einstein himself who introduces gravitation into the experiment by weighing the box and thus making use of the identification of inertial and gravitational mass in this formula. Bohr therefore quite legitimately amplifies on this argument by a further analysis of its consequences, based on general relativity.

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REVIEWS

SCIENCE AND PERSONS

I

PROFESSOR Polanyi's Gifford Lectures[1] for 1951-52 form an extremely interesting and learned book which covers a great deal of ground—science, politics, law, art, philosophy, and religion—all are involved to various extents. But it is an extremely difficult book to review for a number of reasons. First, because it attempts to face up to difficulties which works on the philosophy of science often seek to avoid. These difficulties are all connected with the fact that *persons* are involved in the making of science and also enter into the subject-matter of some of its branches. The process of personification—applied even to inanimate objects—which was so prevalent in the ancient world, obviously had to be abandoned if physical science was to develop. A tradition was thus established which has persisted even up to the time of the rise of a science of persons, so that it is perfectly legitimate to be ornithomorphic in studying persons if you find observations on sea-gulls helpful; but it is quite heretical to be anthropomorphic, even when you are studying *ἄνθρωπος* himself. The upshot of all this is that if we want to know what other people think about persons it is not to science but to literature that we must go. It is also customary to *abstract* from the fact of *participation* of persons in science, even when we are discussing the philosophy of science. It is becoming recognised, nevertheless, that a three-termed relation is involved in the pursuit of science, the three domains of which are: (i) persons; (ii) language used by persons; (iii) the subject-matter to which the language refers. If we abstract from the first and third we are said to be talking syntax—we are dealing only with the language, without reference to the persons using it or to the subject-matter referred to. If we ignore persons and deal with the language in its relation to subject-matter then we are pursuing semantics; and if we take into account the rôle of persons then we are engaged in pragmatics. So far pragmatics seems to be the least studied of these topics, and also to be dominated largely by the behaviouristic attitude which itself in effect abstracts from persons as such. It will be seen, therefore, that Polanyi, in not availing himself of the customary methods of avoiding the notion of person, has set himself a very difficult task. A second feature of this book which creates difficulties for the reviewer is the method of presentation, which is intuitive rather than analytical. Sometimes highly debatable assertions are made with little or no preparation, such as: ' tautologies are necessarily true, but mathematics is not ' (p. 187) or ' we are jointly aware of the retinal image and of the adaptive effort ' [of accommodation] (p. 362); or ' we shall see that biology is an expansion of the theory of knowledge into a theory of all kinds of biotic achievements ' (p. 347). Such an assertion may then be succeeded by another statement which is said to follow from the former one, although the connection between the two statements is not always clear (at least to one dull-witted reviewer) and the following of one from the other far from obvious. Thirdly, the author uses a somewhat unusual vocabulary. For example, abundant use is made of the following words: articulation (not in the anatomical sense); achievement (not in the heraldic sense); coefficient (not in the mathematical sense); convivial (not in the dictionary sense); comprehension, commitment, fiduciary, focal observation, subsidiary awareness, passion and passionate, rational (in several senses). These are all key words, essential to the understanding of the book; but the reader is not in all cases told in what senses they are used. It is hoped therefore that the following notes may be helpful. There is a good index and a dictionary is useful but not in every case. For example, according to the *Shorter Oxford English Dictionary* the word *convivial* means ' of or belonging to a feast or banquet, festive; fond of feasting and good company, jovial,' (as we should expect from the meaning of the Latin word *convivium*). But Polanyi does not use it in this sense at all. In the section entitled ' Pure Conviviality ' of the chapter headed ' Conviviality ' we are told nothing about feasts or banquets, but on page 209 we read of ' the primitive sentiments of fellowship that exist previous to articulation among all groups of men and even among animals. Evidence of the primordial character of such conviviality and of the lively emotions engendered and gratified by its interplay is supplied by the experience both of animals and men.' Also on the same page we learn that ' a newly hatched chicken soon learns to join the flock around its mother and to seek protection under her wings '. On the next page we are reminded that ' The comprehensive scope of convivial relations has been expressed by Köhler in the aphorism that a solitary chimpanzee is not a chimpanzee ', and that ' Companionship among men is often sustained and enjoyed in silence '. The author thus appears to have generalised the dictionary meaning of ' convivial ' by not restricting it to feasting. He is using it in contrast to ' objective ' in one sense of this word. Our attitude towards our fellow creatures is objective in this sense if it is that of a detached observer, outside the system (which includes those fellow creatures) under observation but having no part in it; and it is convivial if it is one of fellowship with them, accompanied by a feeling of ' being in it together '.

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[1] *Personal Knowledge: Towards a post-critical philosophy.* By Michael Polanyi. London: Routledge and Kegan Paul, 1958. Pp. xiv. + 428. Price 42s. net.

The word commitment is used in the dictionary sense of ' engagement that restricts freedom of action'. Thus the author speaks of his 'commitment to personal knowledge' (p. 327) distinguishing between 'the personal in us, which actively enters into our commitments, and our subjective states, in which we merely endure our feelings. This distinction establishes the conception of the *personal*, which is neither subjective nor objective. In so far as the personal submits to requirements acknowledged by itself as independent of itself, it is not subjective; but in so far as it is an action guided by individual passions, it is not objective either. It transcends the distinction between subjective and objective' (p. 300). On page 363 we see how widely this notion also is generalised: '. . . biology has been revealed as an appreciation of commitment. To swallow something in the hope that it may be wholesome is clearly a commitment, and so is every act of seeing things in one particular way. I have suggested . . . that . . . commitment may be acknowledged even at the vegetative level, since it is of the essence of a living organism that each part relies for its function on the presence and proper functioning of a number of other parts . . .. Commitment may then be graded by steps of increasing consciousness; namely, from *primordial*, vegetative commitment of a centre of being, function and growth, to *primitive* commitment of the active-perceptive centre, and hence . . . to *responsible* commitments of the consciously deliberating person. . . . Biology is a responsible commitment which appraises other commitments.'

The chapter headed 'The Logic of Achievement' covers such diverse topics as the distinction between right and wrong working of a machine or the organs of an animal's body; between causes and reasons; between logic and psychology; originality in animals; equipotentiality in embryos; and (strangely enough) logical levels in the sense of the distinction between science and metascience. The use of the word achievement is illustrated by the following passage on page 357:

> '. . . I have distinguished two kinds of biological achievements, namely, (1) achievements performed by the rational concurrence of several parts with fixed functions and (2) achievements performed by the equipotential interplay of all parts of a system. . . . Embryogenesis appears to be a comprehensive achievement due to the rational combination of these two types of rational principles.
>
> The analysis of the process by which living beings are formed corresponds to the logic of achievement, as illustrated by the manner in which we find out how a machine works. . . .
>
> The meaning of experimental embryology is thus dependent on personal knowledge: both in respect of the unspecifiable knowledge of true shapes, and in respect of the appreciation of the process by which highly significant shapes and structures are brought into existence. This situation has caused uneasiness among scientists.

The word *articulate* is used in the sense in which it is used by Edward Fitzgerald in

> And, strange to tell, among the Earthen Lot
> Some could articulate, while others not.

The chapter headed 'Articulation' begins with the types of learning in animals, passes on to the use and abuse of language in human relations and ends with a section on mathematical heuristics. There is an error on page 86 in this chapter. Instead of saying: 'the infinite set $\aleph$ counts all numbers' it would be more correct to say: '$\aleph_0$ is the cardinal number of the set of all natural numbers' ('all numbers' would include real and complex numbers); and 'next larger infinite set' is meaningless; what is meant is 'next larger cardinal number'. Also $\aleph_2$ enumerates the set of all real functions of a real variable, but the set of all curves has cardinal $\aleph_1$.

The ways in which some of the other words mentioned above are used may be illustrated by the following, from the Preface:

> I regard knowing as an active comprehension of the things known, an action that requires skill. Skilful knowing and doing is performed by subordinating a set of particulars, as clues or tools, to the shaping of a skilful achievement, whether practical or theoretical. We may then be said to become 'subsidiarily aware' of these particulars within our 'focal awareness' of the coherent entity that we achieve. Clues and tools are things used as such and not observed in themselves. They are made to function as extensions of our bodily equipment and this involves a certain change in our own being. Acts of comprehension are to this extent irreversible and also non-critical. For we cannot possess any fixed framework with which the re-shaping of our hitherto fixed framework could be critically tested.
>
> Personal knowledge is an intellectual commitment, and as such inherently hazardous. Only affirmations that could be false can be said to convey objective knowledge of this kind.
>
> I have shown that into every act of knowing there enters a passionate contribution of the person knowing what is being known, and that this coefficient is no mere imperfection but a vital component of his knowledge.

The word 'fiduciary' seems to be used in the sense given in the *Shorter Oxford English Dictionary*, 'of the nature of, proceeding from, or founded on trust or confidence'. As examples of its use we may quote:

> *The fiduciary passions which induce a confident utterance about the facts are* **personal,** *because they submit to the facts as universally valid, but when we reflect on this act non-commitally its passion is reduced to* **subjectivity** (p. 303).
>
> A fiduciary philosophy does not eliminate doubt, but (like Christianity) says that we should hold on to what we truly believe, even when realizing the absurdly remote chances of this enterprise, trusting the unfathomable intimations that call upon us to do so (p. 318).

Throughout the book the author seems to be haunted by the rival claims of belief and doubt and by the inescapability of both. On page 109 he says: 'I am attempting to resolve by conceptual reform the apparent self-contradiction entailed in believing what I might conceivably doubt.' The author is opposed to positivistic and utilitarian attitudes towards science while criticising also what he calls *Objectivism*. What he means by this may perhaps be gathered from the following passage on page 16:

> . . . modern man has set up as the ideal of knowledge the conception of natural science as a set of statements which is 'objective' in the sense that its substance is entirely determined by observation, even while its presentation may be shaped by convention. This conception, stemming from a craving rooted in the very depths of our culture, would be shattered if the intuition of rationality in nature had to be acknowledged as a justifiable and indeed essential part of scientific theory. That is why scientific theory is represented as a mere economical description of facts; or as embodying a conventional policy for drawing empirical inferences; or as a working hypothesis, suited to man's practical convenience—interpretations that all deliberately overlook the rational core of science.

On the next page the author speaks of the 'indispensable intellectual powers and their passionate participation in the act of knowing. It is to these powers and to this participation that I am referring in the title of this book as "Personal Knowledge".' He has already given hints of his own attitude.

> To say that the discovery of objective truth in science consists in the apprehension of a rationality which commands our respect and arouses our contemplative admiration; that such discovery, while using the experience of our senses as clues, transcends this experience by embracing the vision of a reality beyond the impressions of our senses, a vision which speaks for itself in guiding us to an ever deeper understanding of reality—such an account of scientific procedure would be generally shrugged aside as out-dated Platonism: a piece of mystery-mongering unworthy of an enlightened age (p. 5).
>
> . . . by this time it should be clear how far-reaching are in my own opinion the changes in outlook that are required in order to establish a stable alternative to the objectivist position. I cannot hope to do more in this book than to exhibit a possibility which like-minded people may wish to explore.
>
> I shall go on, therefore, to repeat my fundamental belief that, in spite of the hazards involved, I am called upon to search for the truth and state my findings (p. 315).

But is not the search for truth in natural science a search in which we never *know* when we have caught up with our quarry? We do not *know* which of our current beliefs are true because we do not *know* what discoveries the future may have in store for us which will require their revision. Is not this the reason why we must be content with belief? On page 313 Polanyi says:

> Only an activity can go wrong, and all activity incurs the risk of failure. To believe something is a mental act: you can neither believe nor disbelieve a passive experience. It follows that you can only believe something that might be false. This is my argument in a nutshell; I shall now elaborate it in some detail.

But in the course of this elaboration we are presented, on page 315, with the following statements which seem (to the present reviewer) to throw the matter into complete confusion:

> Therefore, though every person may believe something different to be true, there is only one truth. This can be substantiated as follows. The function of the word 'true' is to complete such utterances as '"*p*" is true', which are equivalent to an act of assent of the form 'I believe *p*'. [On the next page we are told that] 'The concept of commitment postulates that there is no difference, except in emphasis, between saying 'I believe *p*' or '"*p*" is true'. Both utterances emphatically put into words that I am confidently asserting *p* as a fact. This is something I am doing in the act of uttering the words in question, and is quite different from my reporting that I have done this in the past, or that somebody else has either done this or is doing it now. If I report "I believed *p*" or "*X* believes *p*", I am not committing myself in respect to *p* and hence no utterance linking "*p*" to "true" corresponds to these reports; they issue in no assertion of the sentence *p* as true, be it in relation to my own past or to other people's beliefs. There remains therefore only one truth to speak about.

Polanyi has much to say about the Laplacean intelligence which 'would embrace in the same formula the movements of the largest bodies in the universe and those of the lightest atom'. His remarks about 'the menace to all cultural values, including those of science, by an acceptance of a conception of man derived from a Laplacean ideal of knowledge and by the conduct of human affairs in the light of such a conception' recall what I. D. Suttie wrote about the taboo on tenderness in *The Origins of Love and Hate* and what Aldous Huxley says about organised lovelessness in *The Perennial Philosophy*.

The last of the four parts into which Polanyi's book is divided is chiefly devoted to problems of biology and biological methodology. The notion of person was eliminated from physical thinking at an early stage; how far it, in one form or another, should be retained in biology has been a matter of heated debate for centuries. For a long time orthodoxy in biology has required close adherence to the example of physics both in method and in explanatory hypotheses. It has been much easier to follow a successful model and to borrow ready made hypotheses than to invent new ones suggested by the observed subject-matter. No alternative to this procedure in biology has so far had a sufficiently spectacular success to persuade biologists to follow it. On all these topics Polanyi makes interesting suggestions. His book is a challenge to orthodoxy in many fields. We may conclude with his own statement of the aim of his book which is given on page 381:

> Its aim is to re-equip men with the faculties which centuries of critical thought have taught them to distrust. The reader has been invited to use these faculties and contemplate thus a picture of things restored to their fairly obvious nature. This is all the book was meant to do. For once men have been made to realise

> the crippling mutilations imposed by an objectivist framework . . . many fresh minds will turn to the task of reinterpreting the world as it is, and as it then once more will be seen to be.

But will our politicians with their dangling deterrents and misguided missiles—themselves among the products of the objectivist framework—give us time in which to use these faculties?

J. H. WOODGER

SIR RUSSELL BRAIN ON MODES OF APPREHENSION

THE subject of Sir Russell Brain's Riddell Memorial Lecture[1] is ' Modes of Apprehension ', and the modes discussed are sense-perception, both normal and hallucinatory (chaps. I and II) aesthetic experience, and finally but rather briefly religious experience (chap. III). All these are contrasted with scientific inference, which he sometimes calls ' comprehension ' and sometimes ' indirect apprehension '.

The first two chapters (I: ' Vision and Fantasy ' and II ' The Nature of Perception ') may be considered together. The author's main aim here is to insist that the sense-datum theory is the only one which is compatible with the well-established neurological facts, and also the only one which is compatible with the phenomenological facts about hallucination. His own version of the sense-datum theory, here as in his other writings, is a representationalist theory roughly similar to Locke's.

Quite often, however, he prefers to speak of perceptual objects rather than sense-data. But it is never made quite clear what the relation between the two is. I take it that a perceptual object is some sort of complex or family of sense data, and not just a single sense-datum. It is also private to a given percipient (this is explicitly stated in a number of passages of the book). But now suppose I have two successive views of the same house as I walk around it. Is my perceptual object the same on both occasions? The sense-data are certainly different. If the perceptual object is the same, it would be public, as it were, to different points of view and different occasions of sensing, even though private to a single percipient. Moreover, if there is a temporal interval between the two successive views, during which I have no sense-data of the appropriate sorts (because I am asleep, or looking in another direction) do we still say that my perceptual object is the same on both occasions? If we do, it will follow that possible sense-data, as well as actual ones, must be mentioned in our definition of the term 'perceptual object'. We shall have to borrow some of the analytical techniques of the phenomenalist—and encounter some of his difficulties. Again, the same *physical* object can stimulate different sense-organs. I see and touch a pencil and tap with it on the table. Are these visual, tactual, and auditory sense-data all included within the same perceptual object?

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[1] *The Nature of Experience*, Riddell Memorial Lecture, Thirtieth Series, delivered at King's College in the University of Durham, 1958, bo Sir Russell Brain, Bt., Oxford University Press, 1959, pp. 73; 8s. 6d.

Such questions, no doubt, are more easily asked than answered; and similar questions arise if we speak of 'the perceptual world' (p. 24 and elsewhere) and contrast it with the physical world. They also arise if we distinguish, as Sir Russell does, between private perceptual space and physical space (p. 36 and elsewhere). I take him to mean that each percipient has just one private perceptual space in which all his sense-data which have spatial properties at all are located, no matter what sense-modality they belong to, though my perceptual space would still be different from yours, and both would be different from the space of the physical world. In that case, a perceptual space would be private but inter-sensory: public, as it were, to several different senses of the same percipient, though not public to different percipients. But it might be argued, and frequently has been, that even a single percipient has several different perceptual spaces—a visual one, a tactual one, a kinaesthetic one. Or should we say that each percipient has several different *sensible* spaces but only one single perceptual space, though all of them are alike in being private to him?

These questions again are more easily asked than answered, though Sir Russell is no doubt right in holding that *some* distinction between perceptual space and physical space is essential to the sense-datum theory.

It should be added that his account of private perceptual space has one rather peculiar feature. He certainly holds that perceptual space and physical space are different spaces. On the other hand, he also thinks that we can properly ask the question '*Where* is the perceptual space of each of us?' (pp. 40-41)—and answer it too. The answer is that Mr A's private perceptual space is in his brain: not, of course, in his perceptual brain (if we can properly speak of such a thing) but in his physical brain, inside his physical head. But in that case his perceptual space is just a part of physical space, the part of it which happens to be occupied by his physical brain or by some portion of his physical brain. And then there are not after all two different spaces, or two different sorts of space, perceptual and physical. There is only one—physical space—and the perceptual spaces of the various percipients are just a number of small regions within it. Moreover, when Mr A. is said to have a *private* perceptual space, it now turns out that what is private is not the space itself, but only its contents, the sense-data or perceptual objects which he alone is aware of; and not all its contents either, for some of them are physical objects, e.g. brain cells. The same conclusion is suggested by the analogy of the ship's radar screen (pp. 41-42): 'Here *in a minute space in the vast bulk of the ship* is its (the ship's) private perceptual space' (my italics).

It seems to me that if we wish to maintain that perceptual space and physical space are 'different' spaces (and similarly that my perceptual space is a different space from yours) we must give a stronger sense than this to the word 'different'; namely, a sense which entails that though perceptual objects have spatial relations to one another, and physical objects also have spatial relations to one another, there are no *spatial* relations between any perceptual object and any physical object, whatever other relations there might be between them, e.g. similarity, concomitant variation, causal dependence. In that case the question, 'Where is the perceptual space of each of us?' would be devoid of meaning. At the best it would be a misleading way of asking a causal question, not a spatial one. Similarly, if my perceptual space is a different one from yours, my perceptual objects have spatial relations to one another and your perceptual objects have spatial relations to one another; but there are no spatial relations between my perceptual objects and yours. According to Sir Russell, however, there are. If my physical head is twelve physical inches away from yours, and we are standing back to back, my present visual field is a little more than twelve physical inches away from yours, allowing for the thickness of our respective skulls.

Sir Russell is of course well aware that the representationalist version of the sense-datum theory has been severely criticised by philosophers from Berkeley's time to the present day, and in chapter II he replies to these criticisms. The most damaging one of all is also the most familiar: 'If perception provides us only with images or representations of physical objects, how can we know that physical objects exist to cause such images or representations?' (p. 42, quoting Mr R. J. Hirst). Sir Russell claims that there is a complete answer to it, and his exposition of this answer (pp. 31-42) is perhaps the most interesting part of the book. First, he says, we must be careful to choose the right starting-point for our theory of perception. 'The fact with which we must begin is the fact of knowledge, experience or information. . . . If we start with knowledge or experience, we start with the subject-object relationship already given. We do not need to ask how we become aware of things outside ourselves because it is with that awareness that we begin' (pp. 31 *ad. fin.*-32).

So far, so good. Most contemporary philosophers would agree that this *is* the right starting-point for an epistemological enquiry. But in the very next sentence we leave awareness behind, and our attention is turned to quite a different topic—namely the degree of likeness which there is between sense-data and the physical objects which are their remote causes. In chapter I it was the *un*likeness of the two which was emphasised. The conclusion reached there, on neurological grounds, was that ' the qualitative features of a percept can have no resemblance to the physical object which it represents ' (p. 24). But—I think this is the point—we must remember that their qualitative features are not the only ones they have. They have what might be called structural features as well. So now, in chapter II, we are warned against ' the error of supposing that the sense-data generated by the brain must be *entirely* unlike the physical objects which, by acting upon the brain from the outside world, produce them ' (p. 32). On the contrary, if we consider how the human brain works, and its evolutionary history, we find that one of its most important functions is ' to provide us with an accurate representation of the spatial structure of the external world as well as of our own bodies '—including ' the ever-changing relationship between our bodies and their environment ' (p. 32).

Here we seem to be back in the thought-world of another very distinguished medical philosopher, John Locke, asking much the same questions as Locke asked in his celebrated discussion of Primary and Secondary Qualities (*Essay*, Book II, chapter VIII), and giving much the same answers: with only this difference, that Locke's primary qualities are regarded as structural characteristics of sense-data rather than qualities. Surely it is clear that a mere *de facto* relation of resemblances between certain features of our sense-data and the spatial structure of the external world does not give us all we need, no matter how exact the resemblance is? ' But ', it may be said, ' we agreed to start with the fact that we are aware of things outside ourselves.' So we did. But we seem to have ended with something which makes that fact inexplicable.

Let me try to put this point in a more constructive way. If being aware of things outside ourselves consists in experiencing sense-data which represent them, we shall have to use some phrase like ' giving knowledge of ' in our definition of the crucial term ' representing '. ' X represents Y ' cannot *just* mean ' X is in certain respects like, or very like, Y '. This may be part of what it means, but it cannot be the whole. In addition, we shall have to say that experiencing the resembling entity X *ipso facto* gives us knowledge of the entity Y which X resembles, or at least of Y's spatial structure. To put it in another way, the awareness of things outside ourselves must be, so to speak, built into the concept of ' representing ' *ab initio*, if a representational theory of perception is to work. Perhaps this, or something like this, is what Sir Russell himself intends to say. But he has not said it with either the clarity or the emphasis which are needed, if his version of the theory is to escape the difficulties which Locke failed to solve. There is, however, an interesting passage later in the book which is helpful: ' Assuming that in general the number, discreteness, and movement of objects we perceive are objective, science infers a structure of the physical world such that some perceptual elements are shown to be subjective. The self-contradictory view that begins with objects and ends by making them entirely subjective is a misrepresentation of the sense-datum theory' (pp. 42 *fin.*, 43). Here the first clause of the first sentence 'Assuming that in general . . .' throws light on what was meant earlier by saying that 'the awareness of things outside ourselves' is something with which we begin.

The replies to some other philosophical criticisms are not altogether successful. For example, Sir Russell does not appear to have understood Professor Ryle's reasons for saying that 'seeing a tree is not an effect' (p. 26) and later accuses him of taking an 'introspectionist view of perception' (p. 31)—a strange charge to bring against Professor Ryle, of all people. Professor Ryle's point of course is that seeing is not a process, and therefore not an effect, because 'see' is a success-word and not a process-word. The correct answer to this is, I think, that the word 'see' has other uses besides this success-use. Sir Russell mentions one of them himself in his discussion of hallucinations. He also appears not to have noticed the 'dialectical' character of the remarks which he quotes from Professor Ayer's *Problem of Knowledge* on page 27. Here Professor Ayer is carefully weighing the pro's and con's of the sense-datum terminology. But after doing so, he proceeds to recommend that terminology—not to reject it—and uses it himself in the rest of the book. On the other hand, Sir Russell's reply to what some contemporary philosophers say about hallucinations is both trenchant and instructive, as we should expect from a writer who is an expert both on the neurology and the phenomenology of hallucination; and he justly accuses these philosophers of not knowing enough about the facts which they are undertaking to analyse. There is a very interesting account of some of these facts in chapter I (pp. 10-23) with detailed illustrations from a number of actual case-histories.

Finally, a few words should be said about the other 'modes of apprehension', the aesthetic and the religious. These are discussed in chapter III ('Symbol and Image'), though the last four pages of chapter II serve as a kind of introduction. The aesthetic theory stated in chapter III is greatly influenced by Collingwood's *Principles of Art.* Substantially, it is the Crocean theory that art is the expression of feelings. It is argued that we shall understand this theory more easily if we bear in mind the distinction already drawn between physical objects and perceptual objects. 'The physical object which is a work of art is one thing and the feelings which it evokes are another. But this is not true of the world of perception. . . . In the perceptual world feelings can be *embodied* in concrete form' (p. 49, my italics). I take this to mean that feelings, as well as sense-data, are sometimes actually constituents of perceptual objects. Thus, 'The artist when creating a work of art in his perceptual world, constructs it of his own *feelings* as well as his own visual, tactile, or auditory sense-data'. He does this by physically modifying a physical object, e.g. chipping a piece of stone, or putting physical pigments on physical canvas. 'This physical object is then available to form perceptual representations in the perceptual worlds of other observers, when (if all goes well) they will find similar feelings of their own embodied' (p. 49). Sir Russell does not tell us how his theory would apply to the aesthetic properties of natural objects, though it is very obvious from several passages in the book that he is himself keenly sensitive to natural beauty. Presumably the spectator is here his own artist, so to speak. Unconsciously, without the physical sweat and toil of carving or paint-making, he just constructs a perceptual object for himself out of his own sense-data and his own feelings, and finds satisfaction in this perceptual embodiment of his feelings. The difficulty here is that, in some cases at least, the feelings appear to be themselves already aesthetic ones—for example, the delight which Sir Russell himself felt at the sight of the dolphins and flying fish, so felicitously described on page 2 of this book. When we are trying to give an analysis of aesthetic experience in terms of feelings, our analysis will be circular if the feelings are themselves aesthetic ones.

But although an aesthetic theory of this kind has some plausibility for the visual arts (including abstract painting and sculpture, pp. 52-54) and also perhaps for music (pp. 55-57) it does not seem to apply at all well to the verbal arts, especially poetry. I cannot see that it helps much to describe a poem as a 'verbal image' (p. 58)—any more than it helps to describe a picture as a 'statement' made in paint, which was a favourite expedient with art-critics at one time. The use of the word 'image' here suggests a failure to distinguish two very different senses of the word 'imagine'—imagining *that* so and so (by means of words or otherwise) and imaging, the production and contemplation of mental imagery. Mental imagery may no doubt 'embody' feelings, as sense-data or perceptual objects can. But it is by no means obvious how propositions entertained without belief, which are the objects or contents of imagining *that*, can be said to embody feelings. Perhaps they can, but it must be a very different sort of 'embodying'.

It seems to me that the word 'image' is equally overworked in the brief discussion of religious experience which follows. But the religious, or quasi-religious, attitude to the irremediable transiency of beautiful things, expressed on the last page of the book, is one which commands our interest and respect. Sir Russell finds comfort in the thought suggested by some very moving lines of Walter de la Mare, that the transiency of beautiful things is itself beautiful. I hesitate to cavil at this. But is it obvious that decay is always or even usually beautiful, even if other forms of transiency often are? And what of the 'inextinguishable hope' referred to on page 57, that time itself is 'only a mode of apprehension'? Should we do well to renounce this rather Kantian hope, if we could—though unfortunately we cannot, since it is inextinguishable?

H. H. PRICE

Combination of Observations. By W. M. Smart.

Cambridge University Press, 1958. Pp. xiv + 253. 35s.

The reduction and interpretation of statistical data have always been important in astronomy, and modern statistical theory owes much to early work on the theory of errors by such giants as Gauss and Laplace. The author of this book is Professor of Astronomy at Glasgow, and he has assembled a useful collection of illustrative examples which may help the young astronomer to see the need for statistical methods. Unfortunately, the book claims to be more than an exposition of such methods and to present general principles useful also, say, to research workers in microbiology or population statistics. The treatment of basic principles is too shoddy for this claim to be justified, and among numerous instances in the book which indicate this weakness the following list may be sufficient:

§ 1·11. The definition of density in terms of frequencies is dubious (the notation F(x) for the density function is also unorthodox).

§ 2·09 *et seq.* The optimum accuracy of the arithmetic mean in an appropriately defined sense is never demonstrated.

§ 3·01. Two definitions of probability (the 'Laplacian' and the 'empirical') are given, but no discussion included of the relation of the empirical to the theoretical concept and of the law of large numbers. The lack of adequate theoretical formulation appears also elsewhere. (Cf. remark above on § 1·11; or refer to the discussion on page 47 on the unbiased estimate of the variance, put forward as an approximate formula. The effects of errors in the variance estimate on assessing the accuracy of the mean is never mentioned).

§§ 7·02 and 7·03. Polynomials or Fourier series are fitted to frequency distributions by least squares without taking account of the dependence of the accuracy of any ordinate on its magnitude.

§ 8·04. Moment and cumulant generating-functions are never used in the book, but the correction of an observed frequency distribution affected by observational errors can most simply be presented by means of them, as in effect the author discovers in § 8·11.

M. S. Bartlett

On Mathematics and Mathematicians (Memorabilia Mathematica).

By R. E. Moritz.

Dover Publications, 1958. Pp. vii + 410. 16s.

One would not expect a teacher's handbook of quotations to have much relevance to the philosophy of science, and this little book does not greatly exceed expectations. Yet it is worth a browse. The study of the 'logic of discovery' is becoming a respectable part of the philosophy of physical science, and it is likely that the existing literature on this aspect of mathematics will be enriched. Non-mathematicians whose conception of mathematics derives from Russell's unfortunate quip, 'mathematics may be defined as the subject in which we never know what we are talking about, nor whether what we are saying is true', may find this volume a useful primer of re-education.

Many mathematicians are quite conscious of the element of craftsmanship in their work, and are sensitive to aesthetic aspects of an argument. The better quotes of Chapters VI and XI illustrate this. Stray comments on mathematics by philosophers are neatly listed and indexed. Who knows this one of Voltaire's?

> We admit, in geometry, not only infinite magnitudes, that is to say, magnitudes greater than any assignable magnitude, but infinite magnitudes infinitely greater, the one than the other. This astonishes our dimension of brains, which is only about six inches long, five broad, and six in depth, in the largest heads.

It should be kept in mind that this collection was made nearly a half-century ago, and so lacks any discussion on the revolution in mathematical methods which was getting under way then. It also suffers from the anthologist's lack of deep knowledge of the history of mathematics and its bibliography. For instance, the collection's value in its own terms would have been enhanced if use had been made of Aubrey's 'Brief Lives'. A few quotes from that source would establish beyond all doubt that mathematicians are, after all, quite human.

J. R. RAVETZ

'*Science in a Tavern*': *Diversions on Science in the Making.* By Charles Slichter.
University of Wisconsin Press, 1958. $1.00.

UNFORTUNATELY this intriguing title is limited to the first, and just possibly the second, of this series of articles and lectures. Taverns are mentioned only incidentally in the later chapters and some of the essays are in no way concerned with science. This is a disappointment. Science is public information and it would have been interesting to have followed how, during the course of centuries, scientific ideas were developed in the Public House. Indeed, the first essay on 'The Royal Philosophers', though it deals with only one group of scientists, is by far the most interesting part of the book.

Your reviewer, having recently partaken of refreshment in the course of several all-day scientific meetings in this country (not, of course, of the Royal Society!), has sometimes asked himself and his colleagues why the English scientists of today tolerate such scanty and poor fare. Our ancestors had no such tradition. Professor Slichter quotes several menus of the Royal Society Dining Club; here is one (1747): two dishes fresh salmon, lobster sauce; cod's head, pidgeon pie, calve's head, bacon and greens, fillet of veal, chine of pork, plum pudding, apple custard, butter and cheese. The drinks (except for claret) were not listed but a French visitor leaves us a record that they were not lacking. In one respect only, conditions may not have changed so much. The Frenchman writes: 'In France, we commonly drink only one cup of good coffee after dinner; in England, they drink five or six of the most detestable kind.'

Membership of the Royal Philosophers' Dining Club was, with one exception, limited to men of great distinction; yet, by 1750, the exception was made that any gentleman presenting the company with an adequate haunch of venison might be admitted. Soon afterwards, it was ruled that a sea turtle might be substituted for the venison and thereinafter the claims of other delicacies had to be met. As the author puts it: 'In modern times, the newspapers would probably refer to this as the "honorary membership graft".'

The later essays are much less interesting. Written at various dates during a long academic career, many of them are of ephemeral significance. Moreover, in so far as they refer to science or philosophy they contain nothing that is new. To mark the division of the paragraphs, the publisher has inserted a strange device which, on close inspection, is seen to be a slightly distorted surface-integral sign. This seems to reflect all too well that the matters discussed are summarised at a very superficial level.

Even if the subject matter had been significant, it would have been wiser to write a new book incorporating a lifetime of ideas, rather than merely to reproduce, with hardly any editing, a series of lectures and articles written over so long a span of years.

But the whole book costs only a dollar; perhaps it is worth this for the fun of the first chapter.

G. W. Scott Blair

Les Limitations internes des formalismes. By Jean Ladrière.
Gauthier-Villars, Paris, 1957. Pp. xiii + 715.

This is an encyclopaedic survey of Gödel's Theorem and related results on the undecidability of mathematical statements and the incompleteness of formal systems. Detailed accounts are given of Gödel's original paper (1931), the theorems of Church, Kleene and Rosser, Tarski's semantical researches, and related work by Chwistek, Curry, Henkin, Kalmár, Kreisel, Wang, and many others. The author is plainly a man of unusual patience, willing to go to extraordinary lengths to document his exposition: 46 pages are needed for the bibliography, 42 for a table of symbols, while indexes and similar materials occupy a further 127 pages.

The book is intended for readers with no previous knowledge of mathematical logic (p. ix): its purpose is to provide them with sufficient understanding of the relevant mathematical arguments and conclusions to permit profitable discussion of relevant philosophical issues. I doubt whether the purpose has been achieved, in spite of the author's impressive mastery of this difficult subject. His accurate and well arranged reports remain close to the original versions, with only minor changes in symbolism and in order of presentation. Consequently, little is done to help the beginner to overcome the serious obstacles to comprehension that await him. To take an example: Ladrière says several times that Gödel's undecidable statements assert their own unprovability (' Il s'agit d'une proposition qui affirme sa propre inderivabilité,' p. 93). Although Gödel himself spoke in this way and has been followed by Mostowski and many others, the assertion is misleading unless carefully glossed. For beginners it might be wiser to stress that the undecidable statements belong to arithmetic and are about the integers—and so to avoid troublesome qualms about circularity and self-reference. The correct view is well explained in Rosser's informal exposition, with which Ladrière is of course well acquainted. Again, no informal exposition known to me succeeds in making plain (for the hypothetical novice) the mathematical *Gestalt* of Gödel's climactic arguments contained in the few pages where he finally *proves* the decisive conclusions. Here, too, Ladrière is a faithful reporter: we see the trees all right, but the wood somehow remains obscure.

The tentative philosophical conclusions advanced by Ladrière are disappointing. It is not illuminating to be told that the meanings conveyed by mathematical language are rooted in an experience *sui generis* by which we have access to the domain of mathematical reality (p. 406) or that mathematical existents are not exhausted in their manifestations (p. 443). But then the philosophy of mathematics makes the best minds begin to stutter.

MAX BLACK

Man and Time: Papers from the Eranos Yearbooks, Volume 3. Edited by Joseph Campbell.

Routledge and Kegan Paul, London, 1958. Pp. xx + 414. 35s.

SINCE 1933, ' Eranos ' conferences have taken place nearly every year, at the home of Frau Olga Froebe-Kapteyn near Ascona, on what might perhaps be termed esotericism and exotericism in the East and in the West. The lectures have been published in a series of *Eranos-Jahrbücher*. The present volume is the third of a series of selections in English translation.

The earlier volumes, 'Spirit and Nature' (1945) and 'The Mysteries' (1955) were more fortunate in having reasonably univocal themes. 'Time' is a word in many different types of language, and this collection of papers presents a confusion of tongues as well as a confusion of referents:

> 'Our discussion will deal principally with the relation of art to its epoch; the second part of our lecture will take up the specific relation of modern art to our own time.' (E. Neumann, 'Art and Time').
>
> 'This and this alone will enable us to observe that despite appearances, despite the almost total absence of explicit testimony, the problem of time lies at the very heart of Gnosis.' (H.-C. Puech, 'Gnosis and Time').
>
> 'What is certain is that in progressively decelerating the rhythm of breathing in prolonging the expiration and inspiration, and increasing the interval between these two elements of respiration, the yogi experiences a time different from ours'. (M. Eliade, 'Time and Eternity in Indian thought').

It would be interesting to know how successful was the discussion at this conference (that of 1951) in creating a metalanguage for its purpose. The editor merely remarks 'Our minds at this meeting were prodigiously churned'. An introductory essay, 'The Time of Eranos', by Henry Corbin, is poetic rather than exegetic in this respect.

A paper by Professor Max Knoll, 'Transformations of Science in Our Age', may be selected for comment here. It is in two, not very closely related, parts. The first, 'Growing awareness of typical pairs of aspects in physics, psychology, and other sciences', seeks meaningful analogies between wave/particle, conscious/unconscious, intuitive/observational, form/process, essence/existence and so on; the rewards of this quest decrease to triviality as its successes mount up. The second part, 'Astrobiological, astrophysical, and physical time', assembles some interesting curiosa: sunspot frequency and tree growth; metre waves and cell division in plants; planetary configurations and radio disturbances; 'correlations between the 27-day period of the sun, the frequency of magnetic storms and of northern lights, the death rate, and the rate of incidence of [deaths from] certain diseases'; atmospherics and pigeon orientation; magnetic-storm cycle and birth-dates of Nobel Prize winners. I am not sneering. I am all for a professor of electrical engineering, like Dr Knoll, daring to ride a hobby-horse outside his own field, ignoring the tracks beaten by others, to keep alive a sense of wonder, curosity, speculation ('cavalier/roundhead' !).

One is more inclined gently to mock a specialist's naïvetés in his professed field, as in the third of the passages quoted above. In sleep, in states of excitement, pain, fear, suspense, we all have different experiences of time; whatever there is of interest in mystical experience, it is certainly not this.

G. E. DENYER

The Freedom of the Will. By Austin Farrer.

A. & C. Black, London, 1958. Pp. xi + 315. 28s.

Would that all theologians had as light a touch and as lucid a style as the author of these Gifford lectures! Here is philosophy at its wittiest, well-informed, yet free of bookishness almost to a fault. Nothing here to repel needlessly the restive scientist, anxious to know whether his research on the brain leaves other people's will free.

What will he learn? Certainly, if he is fair, a new sympathy for old-fashioned ways of posing questions. Dr Farrer has no inhibitions from enquiring after such things as the 'seat of consciousness' (as long as we distinguish the psychological from the physiological seat); and when he is done, the reader may see more sense in it than current fashion had allowed. Throughout the book he may have to read rather carefully, for the author delights in protracted (and pretty convincing) advocacy of opposing views in turn, so that random sampling becomes both confusing and dangerous. But on all the usual topics of mind and body, motive and choice, responsibility and freedom, he will find an immensely enriching and stimulating fund of epigram and imagery, and some good arguments.

Naturally, he will be eager to know what such an attractive thinker makes of the causation of bodily action; but it is just here, despite some really excellent ground-clearing, that the scientist may find himself drifting out of contact with yet another metaphysician: which is exasperating, for this one has been fun to follow. Epiphenomenalism is untenable: Good. Parallelism is untenable: Good. Physical laws are not disobeyed by brain-mechanisms: fine. But in the brain 'The minute elements function regularly, yet allow themselves to be overruled . . . (p. 52) . . . the (biological) pattern of action does real work . . . and leads (them) a dance they would not otherwise tread' (p. 60) ('otherwise' in what sense?). And then (p. 99) '(this) "bewitching" of our minute physical energies by organising patterns remove(s) their operations from the sway of physical exactitude'.

Alas—where are we now? Just when the author (having dismissed the Cartesianism of Professor Eccles) seemed ready to grasp the nettle and declare that human agency *is* personal agency (and free at that) whether or not the human brain is physically (exactly) determined in its actions, he introduces *ad hoc* a concept that effectively makes free personal agency subject, as in so many other arguments, to our ignorance of physical causation.

That such ignorance is in practice inevitable we need not dispute. Our question must only be whether it is *necessary*—whether it is needed to make room of the right sort—for the validation of freedom and responsibility, (particularly as conceived by biblical Christian theology). Is not what is minimally necessary reducible to something much weaker—namely, the *logical* indeterminateness, *for the agent*, of the choice he must make? It can be argued (as, I have recently argued, *Mind*, 1960, **69**, 31-40) that such indeterminateness is inevitable even if to the isolated *onlooker* the agent's brain enjoyed no release from 'the sway of physical exactitude'. Until the implications of this indeterminacy have been exhausted, it seems a pity to postulate (without apparent justification) anything more queer and scientifically frustrating. Before we can either assert or deny that 'the occasion determines the form' of a change of mind (p. 165) we must ask 'for whom—for the agent, or for the onlooker?' Determinateness in such a case denotes not a unique *status* of an event, but a *relation* between the event and its contemplator, which is demonstrably not the same for the agent as for the onlooker.

Despite the force that is lost from some of Farrer's arguments when we thus sort out the beliefs that could be valid for onlooker and agent respectively, he makes many points of great value. To cite only one example, it is refreshing on page 269 to find a robust rejection of the common tactic of admitting most decisions to be determined (physically or psychologically), but clinging to the hope that a residuum of sufficiently important choices are exempt. As he rightly sees, normal deciding is not a matter of residual free play in the bearings of an otherwise determinate mechanism: 'Decision as such, any decision, is the foundation of a sound libertarianism. . . . Any libertarian edifice which narrows its base, and builds on privileged decisions . . . is doomed to fall. For once we admit that most decisions are determined, we cannot resist the contention that all may be.'

By contrast, his discussion of the power(lessness) of 'machines' to 'exceed their principles of construction' (p. 286) shows curiously little grasp of current thinking on self-organising and self-reorganising systems. To many of us the limits here seem in principle no more restrictive than those set by human genetic machinery on the capabilities of human beings. If space for more adequate discussion was lacking, this whole topic might have been better omitted; for it is one with which any realistic metaphysics of personality must in the end come earnestly to grips.

An intricate work of art such as this cannot escape misrepresentation in a brief review. The reader is urged to enjoy—and assuredly to profit from—the experience of its direct impact.

D. M. MACKAY

Inside the Great Mirror. By James K. Feibleman.
The Hague, 1958. Pp. 216. Guilders 19.

RUSSELL's new logic raised anew the problem of realism versus nominalism. Russell himself attempted to develop a new nominalist theory of language to fit this new logic. This attempt was unsuccessful. Next came Wittgenstein's attempt, which was also unsuccessful. The theory which this attempt resulted in (the *Tractatus*) is by now almost entirely abandoned;

only one of its corollaries survives, namely that metaphysics is meaningless. It survives, although there is no reason to support it, because of its usefulness for the obscurantist.

This seems to be the crux of Professor Feibleman's story. While narrating it he makes many very interesting remarks on many sidelines or detailed problems. In many respects this book is very good indeed, but I can recommend it only to the reader who can discriminate between the chaff and the grain.

The historian of contemporary philosophy will delight in Feibleman's interpretation of Wittgenstein's *Tractatus* because he takes it very seriously and sees in it an attempt to solve one of the philosophical problems of that period. It is a pity perhaps that Feibleman discusses only one issue of that period and no others; I particularly missed Russell's paradox; but as far as it goes the analysis is good. Readers of this *Journal* will be interested in Feibleman's interpretation more than in others as it is related to problems of the philosophy of science. Anti-metaphysicists will probably hate his jokes and pro-metaphysicists may enjoy them, at least when they are made in good humour. Both parties will be puzzled by his coupling of leniency towards the old Wittgenstein with harshness towards his closest disciples. Feibleman's claim that ' a follower is small just in virtue of being a follower, since he is not a leader ' (p. 152) is unjust. With the possible exception of Wittgenstein and his like, we are all humble followers to some extent or another (Feibleman himself is a professed follower of Peirce and of Whitehead) and lack of originality is no sin. I hope that readers will pay attention to the valuable core of this book and ignore its weaker parts.

J. AGASSI

Sherrington: Physiologist, Philosopher and Poet. The Sherrington Lectures, IV.
By Lord Cohen of Birkenhead.
Liverpool University Press, 1958. Pp. xiv + 108. 17s. 6d.

THIS work in an excellent exposition, mainly factual, but with some perceptive philosophical criticism, of Sherrington's career, and his extraordinarily large number of works on many different subjects (the majority physiological). The section on his philosophy occupies only about 13 pages of a total of 108 pages (which include 35 pages of bibliography), and ends with the admission that in the field of the relationship between mind and body, ' Sherrington's thinking cap was second-hand—it had been worn by all the exponents of dualism for three centuries since the days of René Descartes ' (p. 64).

Sherrington in his experimental physiology (for which he is best known and with most reason ranked as a genius) was concerned mainly with reflex activity, for, he said, 'it can be studied free from complication with the psyche'. He thought that some parts of the brain 'may be called mental'; and that therein 'nerve-actions exist still unknown to us, and that these may correlate with mind. . . . There is, so far as I know, in the chemical, physical properties, or microscopical structure no hint of any *fundamental* difference between non-mental and mental regions of the brain.' Such confusion of use of the words 'non-mental' and 'mental' will invalidate, for many modern readers, other statements about brain and mind, neurology and psychology.

As Lord Cohen expertly points out, Sherrington was aware of the semantic problem attached to such words as 'life' and 'mind', since he discusses them, in part, in relation to the term 'life', 'yet he surrendered to the doctrine of body-mind dualism, and then, failing to unify its disparate entities, re-enunciated its philosophy'.

How curious it is that the neurologist and neurophysiologist, who are, by their work and interest at the junctional point of science and philosophy, in the ideal position to make the grand synthesis, should be so afraid of or disinterested in and insensitive to their opportunity and obligation, that they remain within the narrow field of reflex activity and clinical description. So the philosophy of even the greatest neurophysiologist is justly estimated as 'second-hand'; and neurology contracts like a withered bud, to the dismay of those who perceive its unique position in the search for knowledge and wisdom.

WILLIAM GOODDY

Religion and the Scientific Outlook. By T. R. Miles.
Allen and Unwin, London, 1959. Pp. 224. 21s.

MR MILES maintains: (1) That sentences in which the word 'God' occurs have no literal factual meaning. (2) That consequently they cannot logically conflict with any scientific statement or set of statements. (3) That the most important use of theistic language is the telling of parables, and the appropriate religious policy is one of silence qualified by parables.

Three features of parables are mentioned: (1) They contain empirical assertions. (2) For the parabolic purpose, it is unimportant whether these assertions are literally true. (3) Parables carry a message and may call attention to timeless truths.

The logical crux of this kind of analysis concerns the relation between parable and parabolised, between the story and its interpretation. The logic of 'parable', used in this way, appears to imply the possibility of formulating a non-parabolic, literal assertion which expresses the parabolic message. If so, reasons should be given why such reformulations of the content of the parable are logically incapable of conflicting with scientific statements. In a discussion of the Christian parable of the Creation in this book, the interpretation takes the form of different theistic assertions, a piling of parable on parable. To appeal here to the necessity for religious silence would not meet the point, since the question is whether one has the logical right to speak of parables in contexts where one does not know how to speak non-parabolically.

This lack of clarity about the logic of ' parable ' seems to me to invalidate the parabolist's answer to positivist criticism of religious utterances. But in any case, the thesis of the logical independence of scientific and religious assertions is appreciably weakened by the author's candid admission that scientific discoveries, though they cannot formally contradict the parables of religion, may nevertheless make these appear ' implausible ' or ' inappropriate '.

R. J. Spilsbury

What is Cybernetics? By G. T. Guilbaud.
Heinemann, London, 1959. Pp. 126. 10s. 6d.

One answer of M. Guilbaud's is that Cybernetics is no super-science but an inter-scientific instrument of analysis, an ideal telephone exchange linking up specialisms (with the insinuation that in the bad old pre-1940 days specialists did not get together over problems of common interest). Another, different answer developed in this essay is that Cybernetics is, or may become, part of a general theory of human action and decisions. This uncertainty is ascribed to the fact that Cybernetics lacks, as yet, any unifying principle to give it coherence.

M. Guilbaud's exposition is restrained and readable, and shows awareness of the historical roots of this not very novel phenomenon.

R. J. Spilsbury

Annual Conference of the British Society for the Philosophy of Science

The fourth annual conference of the British Society for the Philosophy of Science was held from 25th to 27th September 1959 at Newnham College, Cambridge, and was attended by about eighty members and guests. Dr M. B. Hesse was conference secretary.

Four symposia were held: ' Scientific Research and the Philosophy of Science ', ' Biology and Physics ', ' Classification, Concept-formation and Language ', and ' Knowing and Being '.

The first session (Dr J. O. Wisdom in the chair) argued whether 'philosophy of science' can be held to refer to any activity not properly subsumed under 'scientific research', and, if it can, whether that activity is relevant to research. Professor H. Dingle mentioned various questions of value and purpose, as well as of method, papers on which were unlikely to be accepted for publication in journals concerned with particular sciences; he suggested that many research workers had something in common with people hacking passages in a thicket, pausing only to exhort others to join them, and to shrug at those who asked where they were going, and why, and whether their tools were capable of improvement. Professor H. C. Longuet-Higgins, on the other hand, argued that of the products of philosophers of science, part was science (and, as such, *was* the concern of scientists), part was philosophy (of concern to man generally), and the remainder was of no use to man or scientist. This aroused some consternation, as members consulted their own credentials and invoked those of others, but in due course a consensus emerged that scientists have to think in some sense philosophically and the comparative study of modes of scientific thought may help them to do so. The conference did continue.

In the second session, Professor J. H. Woodger described an abstractive hierarchy of terms characterised by a one-many relation, and its use as a conceptual framework in biology. With some hierarchies of cells, every cell was a distinct life; with others only the first cell in each hierarchy, elaboration occurring on subsequent levels. Morphology was the study of the arrangement and differentiation of parts; physiology was the study of the existential dependence of parts. Genetics was concerned with the process in which the first member of a hierarchy results from the conjunction of two members of other hierarchies. Dr E. H. Hutten set alongside this framework the sequence exhibited by radio-active decay: each new atom produced was similarly time-extended, stemmed from a parent atom, and consisted of existentially dependent parts, but it was not dependent on its environment analogously to a cell. In elementary-particle theory, explanation might amount to little more than classification, or an enumeration of possibilities, and might have little more predictive power than comparable biological models with their over-determination and multiple-causation. The chairman (Professor C. F. A. Pantin) referred to the occurrence, in biology, of morphological models that are interpretable at more than one level in an organisational hierarchy. In discussion it was suggested that the predictive power of a theory might not always be manifest at the time of its original formulation; it might have to await development of deduction. There was much interest in the relative importance of the past history of an entity in biology and in physics: *historical* existential dependence appears to be a function of complexity.

In the third session (Mr G. Buchdahl in the chair), Miss M. Masterman and Mr R. M. Needham presented the strategy, and some of the tactics, of a method of analysing language by assimilating it to a library classification system in which concepts are arranged on a finite lattice ordered by a single, weak, 'concordance' (inclusion) relation. They further suggested that the formation of scientific concepts is a development of language according to this model. There was some discussion as to whether the method is a technology for mechanical translation, or a science, or a philosophy of language, and it was suggested that it could be viewed as a scientific model of language, containing the partly uninterpreted concept 'inclusion', and capable of being tested by experiments on translation and on analogy-finding.

In the fourth session (Dr M. H. Thorpe in the chair), Professor M. Polanyi presented a way of talking about the primary process of knowing by perception—the pre-articulate act of knowing, which partakes of the uniqueness of the individual percipient, the unspecifiable personal knowledge from which any specifiable, potentially public, knowledge is derived by a process of abstraction. Complex entities were commonly perceived and recognised as wholes before particulars had been identified; the process of discovery, in fact, might be regarded as an alteration of analysis, recognising particulars, and integration, recognising the relations of parts to the whole. Professor Braithwaite suggested that too narrow a view might be taken of specifiability, and that some levels of subjective experience, however vague, could be conveyed by language—for example, ' Oblomov ' conveys the experience of laziness; beyond that, he differed from Professor Polanyi over his use of the term ' knowledge ' for what was unspecifiable. In the subsequent discussion there was some reluctance to focus on this rather undemocratic mode of tacit awareness, and a preference for talking about what *can* be made public—with the implication that the progressive refinement of scientific language tends to eliminate the unspecifiable.

In conclusion, it is perhaps worth drawing attention to the unusualness of a scientific conference at which speakers are not armed with specified and verifiable data but attend primarily to make as explicit as possible how they think, and to receive criticism of the process thereby revealed—especially from those who are not working in the same field. The coherence of this universe of discourse was illustrated by the frequency of reference from one discussion to another, the *esprit d'escalier* from one session often finding its outlet in a later one; its range, by the frequency of spontaneous quotation, not only from Shakespeare and Wordsworth but also from Swinburne (and *early* Swinburne, at that).

G. E. Denyer

ERRATA

In the November 1959 number, p. 256, line 5, for ' lustre ' read ' lustra '.

In the paper ' Probabilistic Independence and Corroboration by Empirical Tests ' by K. R., Popper, this *Journal*, 1960, 10, 315-318, the following three crorections should be made:

p. 315. insert, on the left hand side of the table of possibilities at the bottom of the page, ' $\omega =$ ' before ' $\sim e$ ', so that ' $\omega = e$ ' stands before the lowest bracket.

p. 315, add to the end of line 5 before the paragraph numbered 4, the following footnote 2:—

2. This shows that it is not enough for Professor Leblanc's purpose to outlaw absolute probabilities. What we should have to outlaw is the probabilistic theory of the intuitive idea of *Independence*—a theory which is of the utmost importance for most applications of probability.

p. 317, line 5. ' $-e = \omega$ ' should read ' $\sim e = \omega$ '.

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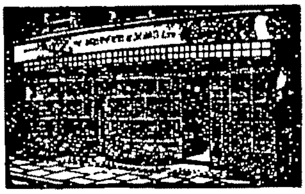

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hierarchy this class has only one member. Members which belong to the converse domain but not to the domain are there called beginners of the converse, but I shall call them the *terminals* of the relation.

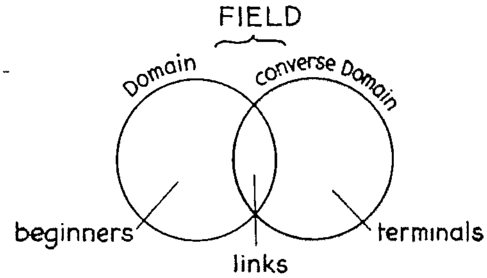

FIG. 1. Diagram of the three mutually exclusive subsets of the field of a two-termed relation: *beginners*, things which stand in the relation to something but have nothing standing in the relation to them; *terminals*, things which have something standing in the relation to them, but do not stand in it to anything themselves; *links*, things which both stand in the relation to something and have something standing in it to them.

Members of the field which belong both to the domain and to the converse domain I shall call *links* of the relation. In an arrow-figure beginners will have an arrow running from them but not to them, terminals will have an arrow running to them but not from them, and links will have at least one arrow running to them and at least one running from them. A diagram of a regular and one of an irregular hierarchy are given (Fig. 2); the former is such that every member of

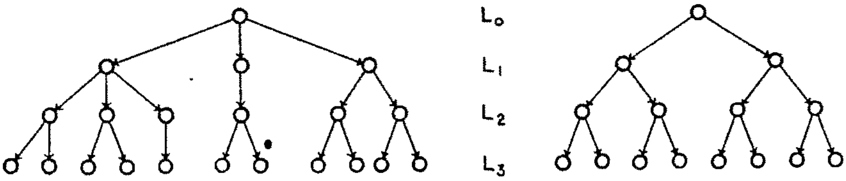

FIG. 2. Arrow-figures of hierarchies; irregular on the left, regular on the right. Numbering of levels indicated by L_0, L_1, etc.

the domain stands in the relation to exactly the same number of members of the converse domain as every other member. Arrow-figures of relations which are *excluded* by the definition of hierarchy are also given (Fig. 3): thus A is not a hierarchy because it has no beginner although it has a finite field. B has no beginner because its domain is included in its converse domain; the dots are intended to indicate an infinite regress. C is not a hierarchy because it is not one-many. D is not a hierarchy because, although it has one and only one beginner, this beginner does not stand in some power of the relation to every member of the converse domain. E is not a hierarchy because it has two beginners. F represents a progression and G a line. A useful notion in connection with hierarchies is that of a *level*. A level of a hierarchy is a set of members of its field to which the beginner stands in the same power of the relation and which includes all the members of the field to which the beginner stands in this power of the relation.

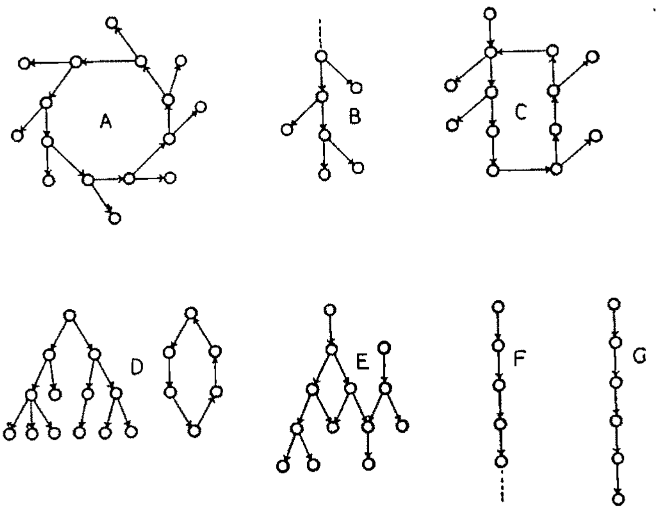

FIG. 3. A to E arrow-figures of relations which are *not* hierarchies: A, because it has no beginner, although it has a finite field; B, because its domain is included in its converse domain; C, because it is not one-many, having a member of its field to which *two* members stand in the relation; D, because although it has but one beginner this does not stand in some power of the relation to *every* member of the converse domain; E, because it has more than one beginner *and* is not one-many. F is an arrow-figure of a progression and G an arrow-figure of a line in the sense explained in the text.

Thus the beginner itself is the sole member of zero level; the terms to which it stands in the relation constitute level one; the terms to which they stand in the relation form level two, and so on.[1]

We can now proceed to hierarchy-generating relations. A relation is hierarchy-generating if and only if by taking any member of its domain and limiting its field to that member and all the terms to which it stands in some power of the relation, the relation so limited satisfies the definition of hierarchy. Thus if the story of Adam in the book of Genesis is accepted as quite literally true then the relation of fatherhood is a hierarchy because there will be one and only one father who is not a child. But even if fatherhood is not a hierarchy it is a hierarchy-generating relation. For if we take any man who has children and limit the field of fatherhood to this man and all his descendants, the resulting relation will be a hierarchy. The female members of its field will all be terminals, together with all the childless males.

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[1] For further information about hierarchies and for explanations of technical terms belonging to relation theory which have been used above see the author's *Axiomatic Method in Biology*, 1937. For a good explanation of the notion of being related by some power of a relation see Quine's *Mathematical Logic*, 1951, p. 215.

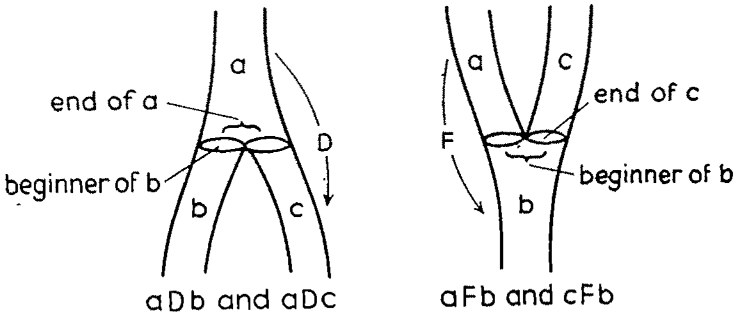

FIG. 4. Space-time diagram of the modes of origin of cells, depicting cells related by **D** and **F**.

Examples of hierarchy-generating relations in biology (apart from the one just given) are provided by certain relations between *cells*. All that it is necessary to know about cells in order to understand what follows is given in the following statements:

(1) Every cell has a beginning and an end in time;
(2) In no cell do beginning and end coincide;
(3) The beginning of every cell *either* (i) is a proper part of the end of a previously existing cell, *or* (ii) is the result of the *fusion* of the ends of *two* cells.
(4) Every cell possesses existentially dependent parts.
(5) Every cell has an environment.

Let us denote by '**D**' the relation in which a cell *a* stands to a cell *b* (see Fig. 4) when the beginning of *b* is a proper part of the end of *a*. And let us denote by '**F**' the relation in which a cell *a* stands to a cell *b* when the end of *a* is a proper part of the beginning of *b*. Then we can say that every cell belongs to the domain or the converse domain of at least one of these relations (see Fig. 5) and that **D** and the converse of **F** (which can be denoted by ' $\mathbf{F}^{-1}$ ') are hierarchy-generating relations. These two relations differ enormously in the size of their fields. The number of links of **D** and of terminals of **D** is colossal and so are the powers of **D** which exist. But links of **F** are comparatively rare; they occur in the embryo sacs of flowering plants but apparently nowhere else; and powers of **F** above $\mathbf{F}^2$ are empty. The importance of **F** seems to lie in the way in which it provides a means whereby terminals belonging to two **D**-hierarchies may unite to form the beginner of another.

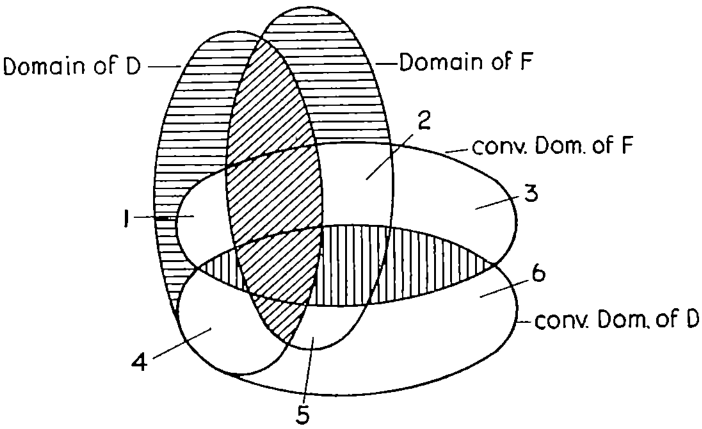

FIG. 5. Diagram of the constituents of the union of the fields of D and F. Sets having no members are represented by shaded areas. Non-empty sets: (1) beginners of D which arise by fusion (called zygotes in animals); (2) links of F (fused polar cells in the embryo-sacs of flowering plants); (3) terminals of F (in animals, zygotes which fail to divide); (4) links of D; (5) beginners of F (gametes in animals); (6) terminals of D other than gametes.

It is not difficult to see a connection between existential dependence and the occurrence of the **D**-relation between cells. If you have a bicycle and you want another but cannot get parts to put together to form one you have no recourse but to saw your bicycle into two halves —splitting each wheel, bar, pedal, etc., longitudinally. If cells have existentially dependent parts new cells will not begin by the coming together of parts but by the splitting of parts of existing cells. Cells are tender things and would have disappeared long ago had they not possessed the power of division. But division alone will not suffice— it would merely yield smaller and smaller cells. We should expect division to be preceded by a process of *duplication* of parts within cells from raw materials *ingested* by the cell from its environment.[1]

Now we must turn to another hierarchy-generating relation among biological objects. Consider first a square. If we bisect each side and join opposite bisecting points by straight lines we have four smaller squares which together compose the square with which we began. The same process can now be repeated with each of the four smaller squares producing sixteen still smaller squares, and so on. Let us denote by '**Sq**' the two-termed relation between any square and each of the four smaller squares into which it is divisible in the above way. Then we can say that; although **Sq** is not a hierarchy it is a hierarchy-generating relation. For, if we take any square—call it *s*—and limit the field of **Sq** to *s* and all the squares to which *s* stands in some power of **Sq** the resulting relation is a hierarchy of which *s* is the beginner. Every square is the beginner of an **Sq**-hierarchy. Moreover, these will be regular hierarchies; because for any natural number *n*, the number of members in level *n* of such a hierarchy will be 4^n. Now, something analogous to this, but much less simple and regular, is to be found in the biological world. The adult human body is divisible into certain parts (called *organ-systems*), for example, the alimentary, the respiratory, the excretory, and other systems, which collectively and in their specific and very complicated anatomical relations compose the body. Each organ in turn is analysable into certain parts (organs), the alimentary system, for example, is analysable into such parts as mouth, pharynx, gullet, stomach, intestine, liver, pancreas, which together in their anatomical relations compose it. Each organ again is resolvable into certain regions or layers which exemplify what are called *tissues*. Thus the intestine has lining layer, a muscular layer, a nervous layer and a covering layer. But of these layers only the lining is specifically concerned with alimentation, the muscular layer belongs to the muscular system which here, so to speak, combines with the alimentary system to ensure movement of the food along the canal. Each tissue is composed of cells of a characteristic kind, together with (in some cases) certain cell-products like fibres or tissue fluids. Each cell is next analysable into two major parts: a central nucleus (some cells have more than one) surrounded by a part called the cytoplasm. Each nucleus is composed chiefly of chromosomes, although there are also non-chromosomal parts; and each cytoplasm has such parts as mitochondria, Golgi bodies, secretion granules, etc. And so on. Certain of the spatial parts of the human body thus fall into *levels* in a spatial hierarchy. Let us call the relation in which the whole body stands to each of its organ-systems or the relation in which an organ system stands to each of its organs or any part to each of its next-level parts (like the relation between square and each quarter-square) the relation **S**. Then although **S** is not a hierarchy each whole body will be the beginner of an **S**-hierarchy.

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[1] The process of duplication prior to division is beautifully illustrated by the protozoan *Euglypha alveolata* which is covered by small shell-plates except for a small hole for the protrusion of pseudopodia. Prior to division a duplicate set of shell-plates is formed round the nucleus. See E. A. Minchin: *An Introduction to the Study of the Protozoa*, 1912, p. 112, Fig. 59.

But instead of talking about bodies we shall speak of *lives* to remind ourselves that we are speaking about time-extended things. Some cells are lives and some are not lives, but the cells which are not lives are proper parts of lives which are not cells. Lives are objects to which *taxonomic names* are applicable, but to the spatial parts of lives such names are not applicable. Thus we say that a man is a member of the species *Homo sapiens*, but we do not say that his big toe or his liver is a member of this or any other taxonomic group. Consequently cells which are lives are also given taxonomic names, like *Amoeba proteus*, the microscopic life which is found in ponds. But cells which are spatial parts of lives which are not cells are not given such names. We must distinguish between unfinished lives and finished lives. A life *a* which is a proper part of another life *b* is an unfinished life if its end is before that of *b* in time. Thus a child of one year old which continues living until it is two years old is an unfinished life. A finished life is one whose end is the beginning of a *corpse*.

In addition to these remarks about lives it is also necessary, in order to understand what follows, to know what is given in the following statements:

(1) Every life has a beginning and an end in time.

(2) In no life is the beginning coincident with the end.

(3) The beginning of every life which is not a cell is *either* (i) the beginning of a cell which is a life, *or*
(ii) a cluster of beginnings of cells which are not lives.

(4) If the beginning of a life is the beginning of a cell that cell belongs to the converse domain either of **D** or of **F** (see Fig. 5).

(5) If the beginning of a life *b* is the beginning of a cell belonging to the converse domain of **D**, then there is a life *a* which is not a cell

from which that cell has been derived by division; the life *a* is called the asexual parent of *b* (Fig. 6 (ii)).

(6) If the beginning of a life *c* is the beginning of a cell *c* belonging to the converse domain of **F**, then there is at least one life *a* which has produced a cell which has fused with another cell to form *c*, and *a* is called a *sexual* parent of the life *c* (and of any life of which *c* is a proper part). (Fig. 6 (iii)).

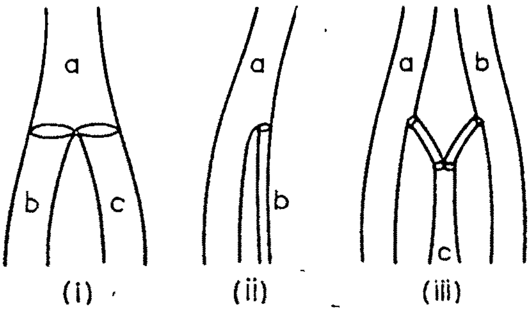


FIG. 6. The three modes of origin of lives which are not cells: (i) division; (ii) budding (if beginning with many cells), or parthenogenesis (if beginning with one cell); (iii) sexual reproduction. In (i) *a* is asexual parent of both *b* and *c*; in (ii) *a* is asexual parent of *b*; in (iii) *a* and *b* are both sexual parents of *c*; *a* and *b* may be one, when both gametes are produced by the same life.

(7) If the beginning of a life is a cluster of beginnings of cells which are not lives there are again two possibilities: (i) a life which is not a cell *divides*, i.e. it ends in *two* cell-clusters *each* of which is the beginning of a life which is not a cell. In this case the first life is asexual parent of *both* of the resulting lives (Fig. 6 (i)); (ii) a life which is not a cell separates off a cluster of beginnings of cells which forms the beginning of the new life but does not itself end until later. In this case also the first life is asexual parent of the second (Fig. 6 (ii)). In case (i) it is customary to speak of one life *dividing* to form two (just as with division of cells); in case (ii) we speak of the parental life *budding* to produce offspring (if the life *b* begins from a cluster of cells).

(8) The cells which are parts of a life which is not a cell fall into at least two mutually exclusive classes such that the members of one class are existentially dependent upon members of the other classes.

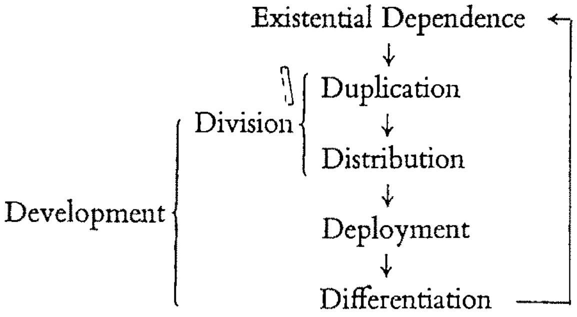
(9) Every life which is not a cell has an environment.

In connection with the different ways in which lives which are not cells may begin it may be of interest to mention that in what is called identical twinning a combination of *two* processes is believed to occur. First a life begins as a result of a *sexual* process and then this life *divides* to form two lives—the identical twins—which are thus the immediate result of an *asexual* process. It is a consequence of this that (contrary to our usual way of speaking) the woman who gives birth to a pair of identical twins is *not* (biologically speaking) their mother but their *grandparent*; for she stands to them in a relation which is the relative product of *two* parental relations. First there is the sexual and then this is followed by the asexual parental relation, and this is a form of the grandparental relation, although neither grandmother nor grandfather.

Returning to our principal theme it will be seen that there are many ways of overcoming the difficulties presented by the occurrence of existentially dependent parts in lives which are not cells. If the spatial hierarchy is not too complicated, so that division does not separate too many cells of one kind from others of another kind upon which they are existentially dependent then reproduction by division or by budding is possible. But among animals it is comparatively rare for lives that are not cells to reproduce in those ways.

Failing division or budding we have the alternative of beginning with a single cell. When a life which is not a cell begins as a cell we say that a process of *development* takes place. This is a characteristic biological process again necessitated—if new lives which are not cells are to appear—by the occurrence of existentially dependent parts in such lives. It consists essentially of the following processes: first *division* repeated until many cells have been formed. This is followed by *deployment* of these cells into masses arranged in anticipation of the establishment of the ground plan characteristic of the type of life with which we are dealing. This primary deployment may be followed by subsidiary processes of similar kind establishing parts of parts, and parts of parts of parts, in the manner of a spatial hierarchy. Finally a process of *differentiation* takes place among the cells, resulting in the production (in the appropriate relations) of secretory cells, muscle cells, nerve cells, covering cells, blood cells, etc. When this is complete division among many D-lines will have ceased and the resulting terminal cells will form the adult body, except in places where there is loss of cells, such as in the skin or among blood-cells. There a stock of link cells must remain from which losses can be made good. Under pathological conditions cells which would normally be terminals may become link cells and result in the formation of cancers and new growths. The study of the conditions which normally lead to the inhibition of division is thus of great importance.

The occurrence of differentiation among the cells of a **D**-hierarchy means that a cell-process distinct from duplication and the distribution of duplicates among new cells takes place. There is a process which we can call *elaboration* yielding cell-parts which can be called *elaborates*—parts which are *not* duplicates of parts occurring in cells standing in some power of **D** to the cell in which elaboration is taking place, but appear now for the first time in the life concerned. The production of different kinds of elaborates in separate cells—muscle-cell elaborates in some, nerve-cell elaborates in others, and so on, leads to differentiation and the establishment of mutually existentially dependent groups of cells. An aggregation of cells all of the same kind does not lead to a spatial hierarchy of a higher level than the cell because there is no existential dependence among the cells. This is well illustrated by the filamentous algae which are simply threads of similar cells. Existential dependence thus results in a kind of cycle of processes:



Before concluding a brief reference must be made to two kinds of parts of lives which are not cells which have not yet been mentioned. These may be called *secretions* and *accretions*. Secretions are parts which begin as parts of cells and end outside cells. These are exemplified by tears, drops of digestive secretions and the hard parts of bones. In most cases the shells of animals are formed in this way. But cases are known where shells are formed from materials picked up from the environment. Such parts would exemplify what is here meant by accretions. The clothing, the houses and the tools of human beings are also accretions; they are parts which have not begun inside the cells of the animal which uses them. Machines are also accretions; they are also existentially dependent upon the persons who make them.

Perhaps the persons who make them are also becoming existentially dependent upon them.

It was pointed out above how the branch of biology called physiology was concerned with existential dependence. Two other characteristic branches of biology are morphology and genetics. Morphology is concerned with the comparative study of spatial hierarchies from the structural point of view. Genetics owes its existence to the existence of the parental relation.

In conclusion we may say that biology is concerned with objects whose parts (or some of them) exhibit hierarchical order in space and are themselves ordered by hierarchies whose fields are extended in time; this type of order being connected with the occurrence in these biological objects of parts which are existentially dependent upon one another.

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PHYSICS AND BIOLOGY *

ERNEST H. HUTTEN

I

TODAY the task is more imperative than ever before to investigate the so-called living sciences and to find out in what respects they resemble or differ from physics. Most of us are in agreement nowadays that we cannot expect, say, biology, to have theories and laws of the same type as physics. We have learned to respect the fact that the model of particles in motion is not adequate for describing living processes. Biologists no longer want to imitate Newtonian physics when they theorise. There remains, however, the suspicion sometimes—perhaps more among philosophers than among biologists—that at least the logical and semantic structure of a biological theory should be the same as that of a physical theory, however much the theories differ in subject-matter, or in the objects they treat of. We have given up the ideal of mechanicism—I do not want to call it 'physicalism' since this has other connotations—but we may still harbour the ideal of a methodological mechanicism. We may still believe, that is, that biological theory should be expressed in terms of a differential equation of the second order like mechanics, though of course the interpretation of the two theories would differ from one another. I think that, while admitting that there is something in this wish, it may seriously mislead us all the same. Logic and mathematics are formal, in the sense that they fit any subject-matter: but what kind of formalism may be used depends on the subject-matter.

Few people nowadays would impose upon biology, for example, the causal pattern as we know it from physics. This is not merely because they have accepted the fact that, in spite of its great success, physics is not the basic theory to which all others can be reduced: physics, biology, psychology, etc., treat of fundamentally different objects. It is also because the causal structure in physics has become

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less simple and less rigid—statistics has loosened the hold which necessity seemed to possess over the realm of inanimate things. Determinism as we know it from the interpretation of Newtonian mechanics current in the last century is definitely out. With it must go the prejudice that the mathematical structure which was believed to represent this determinism, i.e. the differential equation of the second order, has to be employed in biology. From such an equation, given the initial and boundary conditions, a unique solution can be derived; and this solution is then interpreted to describe the unique and necessary behaviour, i.e. an orbit, of a mechanical particle. If we remember that biological and psychological processes are ' over-determined ', that there is ' multiple causation ', then we realise that the search for a differential equation may be very inappropriate in biology. And so on.

I do not wish to talk here about laws in physics and in a ' living ' science—it would be too large a subject. I must and want to confine myself to the concepts which Professor Woodger has chosen for his topic, illustrating the structure of biological theory. This is the whole-part relation and what follows from it, namely, physiology, morphology, and genetics, as constituents of biology. This then is a problem of methodology, of what kind of theory to construct, of what type of logical structures to incorporate into a theory. Can we have a physiology, morphology, or genetics within physics?

Let me say in anticipation of a more detailed argument that, I think, a morphology and genetics, certainly, and perhaps even something like a physiology can be found in physical theory as well. However, we must be reasonable in making the comparison. Usually we compare a biological theory with Newtonian mechanics or even with quantum mechanics, that is, with a highly developed, abstract theory. Naturally we find few if any points of comparison: more often than not we are at a loss to say anything, for we need some common ground which we are hard put to find under these circumstances. My idea is to compare biological theory with a physical theory that is relatively undeveloped and new and therefore simple in structure: this is the theory of radio-activity and, more importantly, the new and quite tentative theory of *strangeness* describing the behaviour of elementary particles.

2

The main fact is, in my view, that biological objects are time-dependent and that biological processes are genuinely dynamical, that is, result in irreversible changes. This is in contrast to the usual physical object, e.g. the Newtonian particle, which is permanent and only suffers space-time displacement. Even if more complicated physical processes are considered, there are always one or more conservation principles. Classical thermodynamics is only thermo*statics*, correctly speaking; for the three basic ideas of closed system, equilibrium state, and reversible path preclude an over-all genuine change of the system (though they allow a partial change as measured by the entropy).

The relation of existential dependence, according to Professor Woodger, is the basic principle of biology. Living things are wholes containing parts that stand in this relation to one another. Thus the whole/part relation is the basic concept. Parts that belong to the same level need each other for continued existence, and this is physiology. The parts also exhibit a spatial hierarchical order, or levels; and thus a morphology comes about. Finally, the wholes are 'time-extended': there is the parental relation between wholes, development occurs; and we have genetics.

If we look at radio-active atoms or unstable, elementary particles, we can *almost* identify them with a biological cell. They satisfy three out of the four conditions Professor Woodger has laid down for cells. (i) Every particle is time-extended and has a beginning and an end in time; (ii) the beginning of every particle is either part of the end of a previously existing particle or is formed by the union of the end of two previously existing particles; (iii) every particle has existentially dependent parts. The fourth condition, that every cell has an environment, does not find an analogue in radio-activity at least, since decay of particles is totally independent of external influence and an intrinsic characteristic of the atom or particle.

There are four radio-active series or families. A parent atom or nuclide produces a daughter-nuclide by means of three possible activities, i.e. α-, β-, γ-decay. This gives rise to a hierarchy, starting from the original radio-active atom and ending in the stable end-product. An illustration is given by the thorium radio-active series.

Physicists were of course always aware that they had taken over their terminology from the biologists. There is also a historical, and often forgotten, connection between cell and atom. For both originally stem from the Greek atom, that is, the basic building-block that is permanent and indestructible out of which everything else in nature is constructed. Moreover, all change is explained by the shifting combination of these building-blocks, so that there never is any genuine destruction or loss. This concept was the outcome of the Ionian school of *physiologoi* from which modern physics, and science as a whole, developed. The cell is the analogue of the atom in the living sciences. And just as the atom, in modern times, showed signs of splitting, became divisible, so did the cell. The split is always in the direction of smaller, but indestructible and therefore final, building-blocks. In physics, we ended up with the electron, proton, and neutron as the only stable, elementary particles; and we had to admit a host of unstable particles, mesons, as well as positrons and neutrinos. In biology, it is, I suppose, the gene that is taken as stable and immutable constituent. Therefore, it is no surprise that such far-going parallelism can be found between radio-active families and cell development.

Indeed, corresponding to the biological evolutions we have the evolution of the earth, or the solar system, or the galaxy, or even of the whole universe that is calculated upon the radio-active decay of elements. Thus the evolutionary aspect and genetics are very similar in physics and biology. There is only this difference: the life-times involved in physics—the half-lives of radio-active or unstable particles—range from microseconds to millions of years. In biology the range is very much smaller.

There is also a spatial hierarchy—a kind of morphology among the atoms. For neutrons and protons make up a nucleus, nucleons and electrons form an atom, atoms combine to molecules, molecules form compounds, or crystals, etc. This relation we see—at least to some extent—in spatial terms. There is only this difference: the spatial structures in physics are very much simpler than those of biology.[1]

Finally, we come to what may be the analogue of physiology in physics. The relation of 'needing' is too anthropomorphic, too antagonistic to the inanimate realm, for us to want to use it. All the same, the combination of atoms into molecules, for instance—that is, the phenomenon of chemical valency—has been sometimes regarded from this viewpoint. Indeed, our terminology in physics has in fact been derived from biology. The alchemists did say that the force of valency represents the need of one atom for the other. We speak today of affinity, e.g. the alkali atom and the halogen atom have a strong affinity for each other—electron affinity—and form alkali halides. It is true, however, that one or the other atom can happily survive without their need being satisfied—unlike the biological case. There is then this difference: chemical affinity can be described in simple terms, as a matter of energy balance and electron exchange which are subject to conservation principles.

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[1] It has been suggested to me that the stars furnish a better example of morphology, i.e. stars, clusters, galaxies, etc., since it is achieved through a classification of more directly observed objects.

3

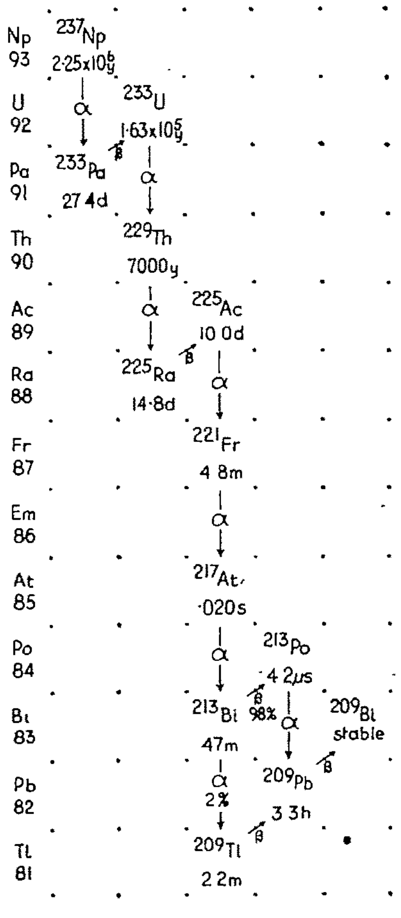
Professor Woodger remarks, at the end of (the first version of) his paper that biology exhibits three main, conceptual difficulties as compared to physics. First, generalisations are almost impossible to make; second, there is little mathematical formalism to bring about a 'scientific' language; third, biologists have not yet learned to formulate their laws as functional relations.

Similar difficulties are encountered, however, if we look at the most recent theory of elementary particles in physics. This so-called strangeness theory is unlike any theory that has ever been formulated in physics; and it is much more concerned with classification and direct observation as biology is often said to be. It is quite impossible, at least at this moment, to imagine any other, more conventional, type of theory for describing the creation and destruction of particles: we seem to have struck rock bottom. Thus it may be of interest to the methodologist to describe this theory briefly.

The great, and quite inexplicable, variety of elementary particles is arranged according to four classes: the heavy baryons, the intermediate mesons, the light leptons, and the photon.

Similarly, the interaction between particles appear to be in four classes. The strongest interaction, among baryons and mesons, is due to nuclear forces; the much weaker, electromagnetic interaction connects all charged particles (and those with electric or magnetic moments) through the mediation of photons; a still weaker interaction leads to the β-decay (of μ, n, K) and the μ-decay (of π- and K-mesons) which involve the neutrino; finally, an interaction of about the same strength as the β-decay leads to the decay of the strange particles.

The relative strengths of these interactions is estimated by the lifetimes of the excited states whose decay they govern. To cut the story short: certain anomalies are found, namely, that the decay of some of the particles is unexpectedly slow—instead of an expected mean life of 10^{-23} sec., the observed value is 10^{-9} sec. The basic assumption is made that these are 'strange' particles that cannot be produced in a collision except in association with other such particles; and that each particle carries away some quantised quantity which the other needs in order to decay. In a nuclear collision this quantity is conserved in its total amount, but its conservation is not rigorous and, indeed, must be violated in the decay process. This quantity is the ‘strangeness’



The neptunium (4nH) radioactive series.

(it is a ‘generalisation’ of the isotopic spin which, in turn, is a ‘generalisation’ of the ordinary spin . . .). If Q is the charge, A the mass number of the particle, T its isotopic spin quantum number, T_s the component of T, and S the strangeness, then: $Q = T_s + \frac{1}{2}A + \frac{1}{2}S$. By choosing T, A, and S appropriately, we can describe the kind and number of particles that may occur in a nuclear collision.

The main methodological problem here is then, as in biology: How is such a simple theory tested? The theory does not predict anything; it does not predict that the result of a collision between two particles is such and such, because there is no way of deliberately bringing about such a collision. The natural decay of unstable particles is not under our control. All we can say is that, having observed a certain, naturally occurring collision, the resultant particles fall into the scheme provided by the theory. Similarly, within radio-activity, we cannot predict *a priori* whether a certain, artificially produced (transuranic) element, for instance, decays into this or that element. We can only find out that, if the decay is of such and such a kind, then the resulting element will be this or that. Or, to take still another example, we do not know *a priori* how to fill the gaps in the Periodic Table (they have, in fact, been filled by now) or to predict which, or how many different, transuranic elements can be produced. All we can say is that, whatever element we find, will fit into the scheme the theory has provided.

I think this situation is interesting, however simple it may be; there are similarities with biology. The standard model for testing a hypothesis, namely, to predict the next occurrence of a known event in space-time, e.g. an eclipse, or a particle in its path, does not apply here. We must, however, distinguish between the actual experimental arrangement and the interpretation of the results. We may, for example, establish the existence of a new transuranic element by means of a mass spectrograph, that is, by observing a beam of particles, or a deposit of particles, in a certain space-time region. That this element exists, and that it does fit into the scheme, that it possesses the properties the Periodic Table predicts, is judged by the consistency of our interpretation of the actual results. The space-time experimental arrangement is merely a convenience: it is not an essential part of the theory. In this particular instance, we have been able to translate or, better, interpret a classificatory theory in terms of the space-time theory underlying the experiment. This is not, nor need it always be, the case. The numerical calculation, the mathematics, belongs to the theory of the experiment, not to the theory to be tested.

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I want to sum up the argument. When we compare physics and biology, then we must take care to choose appropriate theories, that is, theories of the same level of 'abstraction'. In physics as well, there exist simple theories that are classificatory rather than causal (in the ordinary sense of this term). Such theories are concerned, as are the corresponding ones in biology, with genuine processes, creation and destruction. The theories are not very mathematical and seem intrinsically unsuitable for an advanced formal treatment. The theories are not aiming at predicting an event in space-time; they are tested by their coherence or integrative power. Morphology and genetics, instead, replace, so to speak, the space and time relations of the entity described by the theory.

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CHAIRMAN'S COMMENTS ON THE CONTRIBUTIONS BY WOODGER AND HUTTEN *

C. F. A. PANTIN

PROFESSOR WOODGER has drawn attention to the importance of the hierarchical organisation of existentially dependent parts as a characteristic of living organisms. The question then arises, how far is such organisation also to be found in 'physical objects'? Dr Hutten suggests that in part an hierarchical relationship can be seen in a natural transmutation-series of radio-active atoms, though, unlike the living cell, there is in this case no influence of the environment on the process of division.

Some 'physical' systems in fact show an even more complete resemblance to living ones. A drop of ink allowed to fall into a glass of still water sets up a vortex ring. As the ring descends it expands and generates a number of subsidiary rings. These descend and expand like the parent ring; and in due course they may produce a third generation of vortex rings, and so on.

Just as in the case of a living cell, we can say here (1) every vortex ring is time-extended and has a beginning and an end in time; (2) the beginning of every vortex ring (except of course the first) is part of a previously existing vortex ring; (3) every vortex-ring has existentially dependent parts; (4) every vortex-ring has an environment: in this it resembles a cell more closely than does Dr Hutten's transmuting atom.

It may be asserted that such hierarchical systems are not characteristic of 'the usual physical objects'. But reflection about complex physical systems suggests that by 'the usual' is meant 'those usually investigated by physicists'. The tendency to select the simplest systems for scientific analysis can lead to a systematic error in our description of the variety of phenomena unless we are constantly alive to what we have done.

Nevertheless, this hierarchical relationship is such a striking feature of living systems that it is of interest to consider whether there are any

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* Given at the Fourth Annual Conference in Philosophy of Science, Cambridge, 25-27 September 1959.

other features of the relationship which are particularly characteristic of living things. Two occur to me:

(1) In living things, what seem to us important features recur at different levels of organisation. Thus, individuality recurs at several distinct levels; that of the cell, that of the multicellular organism, that of certain colonial organisms (particularly in certain relatives of the corals). In the opposite direction, it can be seen in certain parts of cells, such as mitochondria, which like the cell itself exhibit a hierarchy-generating relationship.

A striking example is seen in certain slime-moulds. In species of the genus *Dictyostelium* the organisms are small amoebae. In the feeding-phase large numbers of these wander about exhibiting amoeboid movement; the pushing out of protoplasmic processes for locomotion and for the capture of food. Under certain conditions these amoebae aggregate together to form a compact sausage-like mass. In this genus the amoebae retain their individuality; they become, as it were, ' cells ' of the large mass. But the large mass itself as a whole then begins to move as a giant ' cellular ' amoeba. Moreover it exhibits directed behaviour comparable to that seen in ' unicellular ' amoebae. Thus we have amoeboid movement and behaviour both at a unicellular (or non-cellular) level and at a multicellular level.

This recurrence of significant characters is of some importance in scientific analysis. Much work is being done on the neurological basis of behaviour; of sensation, of learning and memory, of the directed activities and power to choose. But it is important to remember how many of these features are also evident at the non-cellular level, as in the ' purposive ' behaviour of amoebae. It is clear therefore that these features are not solely the concomitants of the nervous organisation, such as we see in multicellular animals. Any general description of the class of mechanism to which a ' learning machine ' must belong must therefore be independent of the condition of cellular organisation; though it must be expressible in terms of nerve-cell organisation as a special sub-class.

It might well be that we could further restrict the specification of a ' learning machine ' till its requirements could only be met by a machine composed of nerve-cells. But these additional requirements would not be the essential characters of a learning machine.

The properties of a system of synaptic junctions between nerve cells may provide the basis for a learning machine, but we cannot say that the essential feature of a learning machine is such a system of synaptic junctions. The practical importance of this is considerable because we so often find in the physiological analysis of function that a functional need is met not by the development of a single system, but by the simultaneous presence of several systems working on different principles but all meeting the same need. I do not wish now to discuss this interesting matter further: I would only point out that unless its importance is appreciated a biologist may think his analytical task ended when he lights upon the first system in the organism which meets the physiological need he has specified.

(2) The second feature of living things which I wish to discuss is the possession of existentially dependent parts. Dr Hutten suggests that we can see these also in atomic organisation. But there seems to me to be an important difference in the two cases. Characteristically for living organisms we can ask 'What is this part *for*?' and we expect a useful answer. We cannot usefully ask that about the parts of an atom. We can of course ask it about the parts of a machine. If we find a carburettor on a scrap heap we can usefully ask what it is for. From such a part in isolation much can be predicted about the machine of which it is a part. The same is true of the parts of animals: spectacular predictions of this sort have been made from a part of a single bone.

This well known quality of the parts of living organisms is important in the analysis of biological systems. A large class of biological investigations begins with inquiry about the functional purpose of some part. That inquiry is based on anatomical study. The answer to the inquiry is made through a physiological investigation of the properties of the part. When this is made we not only learn something of how the part functions but also see further anatomical implications in the manner of action of the part. Thus, examination of a limb may suggest that it moves in a certain way. Physiological demonstration that it does so may reveal that its manner of action seems to imply the presence of proprioceptive sense organs in certain places in the limb to give information about its position and movement to the central nervous system. The result of the physiological analysis is thus a hypothetical anatomical model. We then return to the anatomical study of the limb to verify this model. That more minute investigation leads to further more detailed specification of function—which is in turn tested physiologically; and so on.

This procedure is possible because the parts of the organism can themselves be divided into existentially dependent parts. Clearly there must be a limit to this, when we subdivide the part down to the molecules, or even atoms of which it is composed.

An interesting illustration of this can be seen in some parasitic organisms. Certain parasitic Crustacea (*Sacculina*) have a larval stage consisting of a sac-like body about 1/16th mm. long containing undifferentiated reproductive tissue. The larva attaches itself to the host (a crab) and this tissue is forced through a special hollow process into the body of the host, to form a growing mass of cells which feed on the tissue of the host. We are not concerned with the rest of the life history. The parts themselves bear a functional relationship to each other and are characteristic of the organism which produces them.

A remarkable parallel organisation to this is to be found at a molecular level of organisation; that of the bacteriophage, a virus parasitic on bacteria. The organism in this case is a sac about 1/10,000 mm. long containing reproductive substance, deoxyribonucleic acid, which is forced through a tube which is attached by specialised threads to the surface of the bacterium. The nucleic acid is forced through the tube into the body of the bacterium, where it multiplies at the expense of the bacterial substance. As machines the two parasites are remarkably similar in spite of the enormous difference in scale of organisation.

But in the phage the parts show evidence of individual molecules of which they are composed. Whereas an isolated part of a large organism can give us unique information about the functional structure from which it is derived, the molecular functional parts can only give us ambiguous information. An isolated molecule of collagen might have been derived from any one of a variety of different structures or organisms.

Nevertheless, these large molecules are in some respects intermediate between unique large functional parts and atoms which tell us nothing of the structure from whence they were derived. Thus if we find molecules of a haemoglobin we can infer with moderate probability that it was the respiratory pigment of a vertebrate animal. We may be wrong, for it may have been derived from other sources, where it had other functions. Thus it might have come from the roots of leguminous plants—where it is concerned with nitrogen fixation. But if all we know about the substance is that it is haemoglobin it is still moderately safe to wager that it was the respiratory pigment of a vertebrate animal.

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THE DOPPLER EFFECT AND THE FOUNDATIONS OF PHYSICS (II) *

HERBERT DINGLE

10 *An Unsolved Problem*

AT present the position is, to put it bluntly, that our conceptions are inadequate to explain the Doppler effect. Whatever theory we adopt, we can imagine situations in which we cannot predict what would be observed. Here is an example.

Consider two exactly similar observers, A and Z, alone in space and relatively at rest at a distance X apart. They carry synchronised clocks, and continuously radiate the same monochromatic light. At the same instant, $t = 0$, they fire identical rockets in the direction Z→A with equal momenta. Will either of them observe a Doppler effect, and, if so, when?

If, according to its definition, a Doppler effect is necessarily associated with relative motion between the bodies concerned, they will not, for at no time is there any relative motion between A and Z. But if there were a third observer, A′, originally at rest beside A, who did not fire a rocket, we have no reason to doubt that they would both observe A′'s light displaced from the moment of firing the rockets. If, then, Z does not observe A's light displaced, it must be that the effect of A's motion on A's light must be transmitted instantaneously to Z, although the observation of A's rocket-firing operation would not be possible to Z until time X/c later. This is difficult to believe in the light of present conceptions, and, in fact, it is often assumed without question [1] that the Doppler displacement would take the same time to reach a distant point as the light itself. Suppose, then, that it does take that time. Then during the interval from $t = 0$ to $t = X/c$, Z, because of his own motion, observes A's light displaced by a definite amount, $d\lambda$, from which he can calculate a velocity dV. What is that velocity? There are only two bodies concerned, A and Z, and they are never in relative motion (A′ is imaginary, and the rockets cannot determine dV, for their velocity is quite indefinite; only their momentum determines the subsequent motion of the bodies from which they are fired). dV must measure a velocity with respect to no physical standard, and the possibility of such measurement is contrary to the postulate of relativity.

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* Continued from the previous Number.

[1] See, e.g. J. H. Fremlin, *Nature*, 1957, 180, 499, and C. G. Darwin, *Nature*, 1957, 180, 976

Still more surprising results follow if we adopt this supposition. Suppose that, at $t = 0$, A alone fires a rocket. Then, up to the time $t = X/c$, Z will go on observing A's light at its earlier frequency. If, then, at that instant he fires his own rocket, he will again be reduced to rest with respect to A, and since he perceives the effect of his own change of motion instantaneously, it will cancel the effect of A's, which otherwise would just have become visible, and all spectrum observations will be to him as though there had never been any relative motion at all between him and A. But A would, in fact, have drawn nearer to him by a distance $dV\frac{X}{c}$, and if, at any later time, he measures the distance of A, he will discover this. If he trusts to the Doppler effect to tell him of his motion relative to A, he will then have to conclude that the only effect of his attempt to recede from A has been to bring A nearer without causing any relative motion at all.

The process could be continued. At the instant $t = \frac{X}{c}$, A also could fire another rocket. Its effect would not be observed by Z until after a further time, $\frac{X}{c}(1 - dV/c)$, and by then A would have drawn still nearer by an amount $\frac{dV.X}{c}(1 - dV/c)$. By a succession of such acts, A could approach to contact with Z, without Z observing any Doppler effect at all, while A would observe one throughout. It would be a sort of Achilles-tortoise race. Z, now revealed as Zeno the Eleatic, could sneer at all the attempts to fault his argument, and triumphantly point to the absence of a Doppler effect as proof that the alleged motion of A was indeed an illusion.

We appear, then, to have to choose between two impossibilities—instantaneous transmission of a phenomenon and observable motion with respect to a non-existent standard.[1] I think that very few, after reflection, would venture to predict with confidence what would actually be observed in such a case: our experimental knowledge is too slight, and our theoretical ideas too imperfect, to permit anything more than a vague feeling of probability. There is, however, one way of escape from the dilemma, and that is to suppose that light itself can act as a standard with respect to which velocities are measurable. This would enable us to allow a delay in transmission of the Doppler effect without invoking a non-existent standard for velocity measurement: our dV would be defined by the fact that the light from A, received by Z at the beginning of his motion, would move with velocity $c - dV$ with respect to Z. But this means adopting the ballistic theory insofar as the velocity at emission of the light is concerned, and rejecting Einstein's second postulate, that light moves with the same velocity with respect to relatively moving bodies. The only way to save both of Einstein's postulates is to suppose instantaneous transmission of the Doppler effect: otherwise one of them is bound to go.

11 *Faraday's Conception of Ray Vibrations*

Instantaneous transmission of anything at all is usually regarded as incompatible with Einstein's theory, though I am not aware that it necessarily violates the basic postulates of the theory, and there is certainly nothing in the ballistic theory to prohibit it. It is, however, inconsistent, so far as electromagnetic phenomena are concerned, with the Maxwell-Lorentz theory, which Einstein's theory was designed to justify. If we allow it to be possible, we are led to a very suggestive line of thought.

The story is well known how Wheatstone, when about to deliver a Friday evening Discourse at the Royal Institution in 1846, was suddenly seized with stage-fright, and at the last moment ran away, leaving Faraday with the duty of entertaining an expectant audience at a moment's notice. It is on that account that, ever since, an assistant has stood unobtrusively between the stairs and the lecturer when he is about to enter the auditorium. What is not so well known is what Faraday said. He did the only thing possible: he gave expression to ideas that were at the moment passing through his mind, which he called 'thoughts on ray vibrations'. He presented them with much diffidence, and very tentatively. As he wrote afterwards, 'I do not think I should have allowed these notions to have escaped from me, had I not been led unawares, and without previous consideration, by the circumstances of the Evening on which I had to appear suddenly and occupy the place of another.' These notions are preserved for us in a letter which he wrote to Richard Phillips, which was published in the *Philosophical Magazine*.[1] It must be remembered that at that time the luminiferous ether was held to be as unquestionable as Einstein's theory is now, and publication of such heterodoxy as Faraday's was possible only in a journal which then merited its title and whose function has now to be performed by such journals as this.

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[1] In a letter to *Nature* (1957, 180, 1275) I described a third result, which involved equal finite delays at both ends in observing a change of relative motion, however caused. This would satisfy the postulate of relativity, but the argument leading to the amount of the delay presupposed that the spectrum frequency was identical with the frequency of reception of light elements. This we have now seen to be unjustified. There is therefore now no ground for accepting that result, and the only delay compatible with relativity is zero.

In Faraday's view, the 'ultimate atoms' of matter are centres of force only, and do not have 'a definite form and a certain limited size. . . . That which represents size may be considered as extending to any distance to which the lines of force of the particle extend: the particle indeed is supposed to exist only by these forces, and where they are, it is.' Light and such vibrations 'occur in the lines of force which connect particles, and consequently masses of matter, together. . . . I do not perceive in any part of space, whether (to use the common phrase) vacant or filled with matter, anything but forces and the lines in which they are exerted. . . . The view which I am so bold as to put forth considers, therefore, radiation as a high species of vibration in the lines of force which are known to connect particles and also masses of matter together. It endeavours to dismiss the aether, but not the vibrations. . . . The aether is assumed as pervading all bodies as well as space: in the view now set forth, it is the forces of the atomic centres which pervade (and make) all bodies, and also penetrate all space.'

This suggestion met with the inaudible response usually accorded to what is unfashionable, and the echoes have reverberated with the same amplitude ever since. Faraday's extraordinary instinct took him beyond what it was possible to apply at that time, and when, many years later, a formal electromagnetic theory of light did appear, it was built on the conception of a fixed universal ether, which has had slowly and painfully to be abandoned, leaving a system of equations which indeed fit observations so far as they can be tested, but lack support in conceivable physical foundations. But at the viewpoint from which we can now survey the Doppler effect, the ' ray vibrations ' take on a new significance. Suppose that each atom (we need not make thought more difficult by refusing to *imagine* the atom apart from its actually inseparable entourage) is the origin of ' lines of force ' (let us call them merely ' rays ', now that ' force ' has a more specific meaning than in Faraday's time) proceeding outwards in all directions. No relative motion is possible between an atom and its rays: if we regard the atom as moving, its rays move instantaneously with it—which, of course, must necessarily follow if, in accordance with the postulate of relativity, we are always entitled to choose a coordinate system in which the atom is at rest. A beam of light ' proceeding from the atom ' consists of a wave travelling along a ray with an invariable velocity c—invariable, that is, with respect to the atom and to the ray; its velocity with respect to a receiving body would be, on the ballistic theory, the resultant of c and the relative velocity of receiver and emitter, and on Einstein's theory, c (this is unpicturable, of course, whatever we may assume).

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[1] *Phil. Mag.*, 1846, 28, 345

On this view, transmission of the Doppler effect would necessarily be instantaneous. Since the ray is, in effect, a part of the emitting atom, it moves with respect to the receiver at the instant at which the emitter moves, and the light waves which it carries, having always the velocity c with respect to the ray, have the velocity $c - dV$ or $(c - dV)/(1 - cdV/c^2)$ (according to the theory we adopt) with respect to the receiver, exactly as though it were the receiver, and not the emitter, which had moved at that instant. But, of course, a Doppler effect would not be perceived unless the ray carried waves, at the position of the receiver, that had been emitted some time earlier. If a new star flares up, we perceive a Doppler effect, corresponding to the expansion of its atmosphere, simultaneously with a sudden accession of brightness. Both these events must be assigned to an instant perhaps many light years before. When the rays from the atoms in the expanding atmosphere moved at the time of the outburst, there were no light-waves on them, at the position of the Earth, by which the motion could be perceived. Such weak waves as were there were lost in the (very faint) light of the star as a whole, which did not change its motion when the outburst occurred. It is quite otherwise, of course, in the case of a binary star, for example. There the components have long been visible, and their rays move to and fro with respect to the Earth, giving us the *present* motion of the star. That is why de Sitter's attempt to disprove Ritz's theory was irrelevant.

In terms of this conception we can form a much clearer idea of what we might expect to observe if the rocket-firing experiments could be performed. The Doppler effect being observable instantaneously at both A and Z, no shift of spectrum lines would occur at either place in the experiment in which the rockets were fired simultaneously. In the Achilles-tortoise experiment, both A and Z would observe the same effect at all stages of the process—namely, a displacement to the higher frequency side throughout. Zeno would be confounded once more. The postulate of relativity would be completely satisfied in both experiments: where there is no relative motion there would be no spectrum displacement, and where there is relative motion the spectrum displacement would yield the corresponding velocity at all times and would be perceived by both observers alike.

Of course, it is not necessary to suppose that the rays 'exist' in the sense that they admit of independent detection by observational means or that it would be meaningful to ascribe to them physical properties such as tensile strength and so on. The philosophy of science of the present day is inconsistent with the naive realism that compelled nineteenth-century physicists to add the metaphysical endowment of 'existence' to the qualities that alone are necessary to justify the introduction of a concept into physics. The rays are kinematic entities only, with no more 'substance' than space or time—unless, indeed, further experience makes it appropriate to amplify their definition. We may, with Faraday, regard them as the only entities in the universe, displacing the conception of point-atoms by that of indefinitely extended 'atoms', and substituting for the axiom that no two bodies can occupy the same space the axiom that all bodies occupy all space; or we may hold both conceptions together, regarding atoms as entities conforming to the first axiom and rays as entities conforming to the second—'atom' being merely a synonym for the origin of electromagnetic waves. This is entirely a matter of convenience, and at the moment the latter view seems to hold the advantage. If, for instance, we have to distinguish between the frequency of reception of light and the spectrum frequency, in the manner suggested earlier, our problem seems better expressed as that of determining how a wave on a ray from one atom interacts with another atom when they meet, than as that of distinguishing between the initial and terminal points of a light-wave in a homogeneous system of rays. But no matter of fundamental principle is involved.

12 *Summing-up of the Situation*

It is not the purpose of this paper to develop the consequences of this possibility, which may well be very great. In cosmology, for instance, the motions observed in the distant nebulae—if indeed the spectrum displacements do correspond to motions—would be those existing at the present time and not at earlier epochs in the history of the universe. But the point of philosophical interest is what we can learn from the Doppler effect concerning the fundamental nature of motion, and the conclusions we have reached may be summed up in the following way.

Accepting the Doppler effect as an experimental fact, two alternative theories are available for its explanation—Einstein's special theory of relativity, and the ballistic theory of Ritz in a somewhat amplified form. Einstein's theory is based on two postulates—the postulate of relativity, which says that motion is essentially a relation between two or more bodies and is a meaningless term when applied to a single body; and the postulate of constant light velocity, which says that all measures of the velocity of light with respect to a body, whatever the motion of that body with respect to anything at all, will yield the constant value c. The ballistic theory also may be based on two postulates—the postulate of relativity, and a different postulate of constant light velocity, which says that the velocity of light is always c with respect to the body (in the last resort, the atom) that emits the light, no matter how that body may be moving at the time of emission or later.

Einstein's theory of necessity makes a complete divorce between the velocity of light and its frequency, because the velocity is independent of the motion of source or receiver and the frequency varies systematically with the relative motion of those bodies. It yields a formula for the Doppler effect agreeing with observation, within the limits of experimental error, on the added assumption that light consists of electromagnetic waves obeying the Maxwell-Lorentz equations, but unless it assumes that a change of relative motion between two bodies that are receiving light from one another is made *immediately* evident to both of them by a Doppler effect, one of its postulates must be violated. If it does make that assumption, the Maxwell-Lorentz theory is violated. But without the Maxwell-Lorentz or some alternative theory, a Doppler formula cannot be derived. It follows that the Doppler effect provides us with a phenomenon incompatible with the requirements of Einstein's theory as it is at present understood.

The ballistic theory, on the other hand, supposes a definite connection between the velocity and the frequency with which light is received from an emitting body. The Doppler effect formula resulting from that connection, however, is only approximately identical with that yielded by Einstein's theory, which appears to accord with experiment, but agreement is reached if a simple relation, not that of identity, and with no theoretical basis yet known, is assumed between the frequency of reception of light and the quantity having the dimensions of frequency which is derived from spectrum observations. Like Einstein's theory, the ballistic theory has its first postulate violated by a delay in transmission of the Doppler effect, unless light can act as a reference body for the estimation of velocities; but, unlike Einstein's theory, it could allow light so to act by the deletion of the words ' or later ' in its second postulate. This, however, would imply that the source could change its velocity with respect to the light between emission and reception, and, further, it would imply a standard of rest entitling one to ascribe that change to the source, which is contrary to the first postulate. Furthermore, if the Earth be chosen as such a standard, and the velocity of light be assumed constant with respect to it, the binary star phenomena cited by de Sitter prove that the modified postulate is not in agreement with fact. Hence immediate transmission of the Doppler effect is necessary on this theory also. This is provided for by Faraday's conception of ' ray vibrations '. With this supplement the ballistic theory gives a satisfactory account of the Doppler effect, but at the expense of the Maxwell-Lorentz electromagnetic theory.

13 *General Comparison of the Theories*

Einstein's special theory of relativity and the electromagnetic theory lie near the foundations of theoretical physics. Their rejection in face of their remarkable triumphs may well seem inadmissible, and is certainly not to be lightly suggested. There is, however, other evidence pointing in the same direction. I have shown elsewhere[1] that the experimental results which have hitherto been held to confirm Einstein's special relativity theory are equally consistent with what I have here called the ballistic theory, if the velocities which in those experiments have been represented by V (and, in fact, have been calculated from the electromagnetic field equations) are really $V/\sqrt{1 - V^2/c^2}$. This would entail, of course, a re-interpretation of the field equations: this will be discussed in more detail presently.

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[1] *Bull. Inst. Phys.*, loc. cit.

The ballistic theory of the present paper is entirely conformable to this change. Its application to the Doppler effect takes us a step towards the amendment of the electromagnetic equations by the picture which it evokes of the ' ray vibrations ' envisaged by Faraday so long ago. The Maxwell-Lorentz equations were constructed on the basis of a fixed universal ether. By common consent, that basis is now regarded as illusory. The equations have been retained because of their consistent agreement with experiment for low velocities but no physical principles are known from which they can be derived: that agreement is preserved in the proposed change. It is certainly a task for the mathematical physicist now to attempt the construction of an electromagnetic theory based on the idea of ray vibrations instead of that of an ether which it is no longer possible to credit.

14 *The Nature of Motion*

The most urgent need is, of course, an experiment to determine whether, in fact, light emitted from relatively moving bodies travels at a single velocity through space. That would at once settle the issue between the two forms of the velocity of light postulate which distinguish the theories, in a manner that would carry conviction to physicists. At the same time, more general considerations belonging to the philosophy of science are relevant in order that the experimental results shall be not only accepted but understood.

The physical phenomenon with which we are concerned in kinematics is motion. As a phenomenon, motion is always a relation between two or more bodies; hence the postulate of relativity of motion is merely a truism. To speak of the motion of a single body is either to imply the existence of another which is not mentioned, or else to use the word ' motion ' to represent something of which we have no experience. Until recent years it was not uncommon to speak in physics of the motion of a single body, and the tacit assumption was that of a universal ' stationary ' ether. The ' relativity ' that has come into prominence during this century is simply the denial that such an ether exists and the assertion that reference to another *piece of matter* is necessary to give meaning to the statement that a single body is moving: it is not a re-definition of motion in relative instead of absolute terms.

The basic problem of physics is to describe the observed motions of bodies in terms that conform to this essential quality of relativity. To do this we define two concepts—space and time—prescribing processes which we regard as measuring certain intervals in them, and measure motion by *velocity*, which we define as the amount by which the position of a body in space changes in a given time interval. Space is conceived as entirely featureless, so that a single body can be said to remain always at the same place, or to be constantly changing its place in any way imaginable, without making the slightest difference to anything observable. This is sufficient to ensure the relativity of motion, no matter whether the *time* between two events in the history of the body is conceived as determinable absolutely or not. If we cannot assign a particular position to a body, then necessarily we cannot assign a particular *change* of position (i.e. a particular motion) to it. We are thus free to define the times of events at different places in any way we please, provided that the definition conforms to our own direct experience of time, without the corresponding conception of velocity violating in the least the essential relativity of motion.

The realisation of this fact was Einstein's great contribution to the subject, for which, whatever the fate of his theory, our debt to him is incalculable. Previously it had been taken as compulsory that time must be defined in such a way that the times of events at different places were uniquely assignable. Einstein pointed out that this was not necessary, and that we were free to define it otherwise if thereby we could attain to a simpler or more accurate description of the way in which bodies naturally move. He did in fact, define it otherwise, and the difference between his theory and the ballistic theory lies simply in the fact that (the time-measuring instrument and the zero-point having been agreed upon) in the former the time-interval between events at different places is indefinite, depending on the arbitrary way in which one regards the bodies concerned as moving, whereas in the latter it is fixed and independent of that movement.

Let us see how this works out in practice. Consider two events in the history of a single body alone in the universe. Whether these events occur at the same place or at different places is entirely a matter of arbitrary choice. We can suppose the body at rest, in which case the space-interval between the events is zero, or moving with some finite velocity, in which case its magnitude will depend on the velocity that happens to be chosen. The time-interval, however, is not so arbitrary. The condition that time measurement must conform to our experience of what we call the continuous passage of time prohibits us from supposing that the time-interval is zero, for the body may be ourselves. If it is provided with a standard clock of the accepted type, that will, of course, give definite readings for the events. On the ballistic theory, the difference between these readings—$\triangle s$, say—is the unique time-interval between the events, but on Einstein's theory it is not: it is the time-interval only if we suppose that the body has not moved between the events. If we suppose that it has moved a distance $\triangle x$, then the time interval must be defined as $\triangle t = \sqrt{\triangle s^2 + \triangle x^2/c^2}$, where c is the velocity of light.

This seems an unnecessarily complicated choice in a matter in which we have complete freedom. Since space and time measurement is entirely a matter of definition, these quantities being concepts in terms of which to describe phenomena and not natural phenomena themselves, one would have expected the simplest choice to have been made. But what is simple in origin may involve great complexity in application, and Einstein's choice was made in order to preserve the simplicity of the electromagnetic equations, in which $\triangle t$ is the measure of time implied. But will this choice allow a consistent description of ordinary kinematical phenomena? Einstein showed that it would, *if it be assumed that the motion of light in space is independent of the motion of the emitting body.* Now in so far as the emitting body can be regarded as moving in any way we like, this is literally obvious, for we cannot suppose that the light will change its behaviour in any way when we change our minds about the motion of the emitting body. But Einstein's assumption meant more than this. If we have two bodies in relative motion, we can assign what velocity we like to either of them, but the other must then have a different velocity: we cannot reduce them both to rest by any act of thought. Einstein's assumption is that the light from two such bodies will, in fact, travel through space with the same velocity. The assumption of the ballistic theory, on the other hand, is that the velocity of light with respect to its source is always the same, and therefore that the light from the two relatively moving bodies will not travel through space with the same velocity.

This is not a difference of definition, but a matter for experiment to settle.

But I think that it is unnecessary to await the result of experiment: we can see that there are internal inconsistencies in Einstein's theory which, whatever result the experiment might give, make it untenable, at least in its present form.[1] This does not mean of course, that the ballistic theory is necessarily right: some third possibility, not yet envisaged, might be the truth. To see the inconsistency of Einstein's theory, consider the following case.

Two observers-cum-clocks, A and B, relatively at rest at a distance $\triangle x$ apart, confirm by light-signals that the clocks are synchronised: that is to say, each one observes that if he sends a beam of light to the other and receives it back by reflection, the time which it shows the other to read at the moment of reflection is the arithmetic mean of the times by his clock of emission and return. The criterion that the observers are relatively at rest is that their positions on a space-measuring instrument, reaching from one to the other, remain constant. That it is possible for A to be synchronised with B and B also to be synchronised with A in this way is, strictly speaking, an assumption, but its truth can scarcely be doubted, it is common to both Einstein's and the ballistic theories, and I shall take it to be beyond reasonable doubt. At an instant when both clocks read o, A and B begin to move towards one another, and eventually meet. The motion may be regarded in either of two equally valid ways, among others. It may be supposed that A remains at rest and B moves towards him, or that B remains at rest and A moves towards him. Suppose first that A remains at rest, and consider the interval between two events in B's history, simultaneous to B with the starting and ending of the motion. The distance between these events is $\triangle x$, and if $\triangle s$ is the reading of B's clock on arrival at A, then the time of the journey must be $\triangle t = \sqrt{\triangle s^2 + \triangle x^2/c^2}$. To A, the time of the journey is the time between the moment when his clock read o and the moment when B reached him. During that time his clock remained at rest, so it records the actual time of the journey, namely, $\triangle t$. Hence, since $\triangle t > \triangle s$, A's clock must be in advance of B's clock on meeting.

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[1] Inconsistencies, that is, in the sense that the theory requires incompatible observations when applied to possible physical situations. Considered as a mathematical theory alone, it shows no inconsistency. It is because attention has been focused too exclusively on the mathematics of the theory that these inconsistencies have been overlooked.

Now clearly we can repeat this argument with A and B interchanged, and in that case B's clock can be proved to be in advance of A's when they meet. These results are contradictory, and it is obvious that the only possibility, consistent with the postulate of relativity—i.e. with the postulate that we can regard either clock as remaining at rest throughout the process—is that the clocks shall agree on meeting.

This objection to Einstein's theory has, of course, not passed unnoticed. A way of escape was first indicated by Einstein himself[1] and has since been explored in more detail by other writers, the latest being Born and Biem.[2] The argument is this. The contradictory results are reached by assuming that complete symmetry exists with respect to the two observers. But in fact (gravitation being neglected, as it has been in the foregoing considerations) A and B, if left to themselves, would never come together. We must apply a force to one of them to make this happen, and although the *motion* of A may be equivalent to that of B, the one that suffers this force is thereby distinguished from the other, and we have no right to suppose that this difference will be without influence on the clock readings. Not that the impulse itself necessarily changes the reading of a clock. That effect, if it happens, will depend on the nature of the impulse, and is incalculable without detailed specification; it may be ignored for our purpose. But the asymmetry affects the problem in a much more subtle way.

Suppose that A is subjected to the force. Then, without introducing any further supposition, we can say that A moves, and so arrive at the conclusion that A's reading will be behind B's when they meet. But if we say that A, although experiencing a force, remains at rest, and B, which experiences none, moves towards A, then we must suppose that a gravitational field acts on the system during the short time in which the force operates, and this just counteracts the force and keeps A at rest while it impels B, which has nothing to resist it, towards A. Without this supposition it is physically impossible for B to move as the result of an impulse applied to A. But now, A and B, being at different places, are in regions of different gravitational potential, and Einstein's theory of gravitation requires that in these circumstances B will gain with respect to A during the time of operation of the field.

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[1] *Naturwiss.*, 1918, **6**, 697

[2] M. Born and W. Biem, *Proc. Kon. Ned. Akad. v. Wetensch. Amsterdam*, 1958, B61, No. 2, 110

Calculation shows that, to a first approximation, this gain more than counterbalances the loss which B experiences on account of its motion and requires that when B and A meet, B will be in advance of A by the same amount as is calculated on the supposition that A is the moving body. The result, then, is that in such an experiment as this, the clock that experiences the physical force will be behind the other when they meet, and this agrees with calculation no matter which clock we suppose to have moved.

This most ingenious explanation, however, though mathematically sound for small velocities, fails completely if we try to regard it as representing what physically takes place. Suppose a third clock, C, to be at rest beside A and synchronised with A and B before the process begins, but suppose that it does not receive the impulse applied to A. Then it will, of course, remain permanently at rest with respect to B, no matter how we regard whatever motion takes place. When the supposed gravitational field causes a rapid relative change in the readings of A and B, therefore, it must cause the same relative change in the readings of C and B, for the difference of gravitational potential is exactly the same for the two pairs. But that means that two clocks, B and C, relatively at rest, to neither of which is anything done at all, must suddenly go out of synchronisation at the moment at which a force is applied to a third clock. This is quite incredible. The fact is that the supposed gravitational field is a pure fiction, which can have no effect whatever on the clock readings; and indeed, when we make the calculations more precise it becomes clear that this explanation not only does not give exactly the right advancement of B with respect to A, but in some cases requires that B must gain large amounts—several years, in fact—over A during the application of a force which may be operative for only a short time, perhaps a week.

No other escape from the difficulty, so far as I know, has ever been proposed. We are bound to conclude, therefore, that Einstein's theory is inconsistent with the postulate of the relativity of motion.

15 *Physical Measurements and Mathematical Symbols*

It is important to understand quite clearly what this means. It does not mean that the theory is 'wrong', in the sense that it necessarily gives a false description of phenomena. It does mean that the relation between the mathematical symbols of the theory and the quantities we measure with our accepted instruments is wrongly conceived. It has been assumed that the Δt of Einstein's theory is identical with clock records of time intervals in certain prescribed circumstances. That leads to contradictions, which can be removed if, in those circumstances, the clock is held to record Δs — i.e. $\sqrt{\Delta t^2 - \Delta x^2/c^2}$—and not Δt. As a consequence, measured velocities are to be identified, not with the quantity $V = dx/dt$, but with $W = dx/ds$, and it is easily seen that $W = V/\sqrt{1 - V^2/c^2}$. So long as we recognise this, it is entirely a matter of physical convenience whether we express our results in terms of dt and V, or of ds and W. The measurements we make in physics are the results of freely chosen operations. We can perform whatever operations we wish, so far as nature is concerned, but the relations between their results, which we call laws of nature, are not then at our choice.[1] We naturally choose the operations whose results are found to stand in the simplest relations with one another, but we are not bound to take those results exactly as they stand and define them as physical quantities to be represented by single symbols in our equations; and, in fact, we do not always do so. We measure ' temperature ' by some physical instrument, for example, but always ' correct ' its readings to what we believe a different instrument would give if we were able to make it. That is quite legitimate, so long as the correction is precisely defined. In the same way we are free, if we wish, to define ' time ' as the reading of a clock modified by combination with a function of the reading of a space measuring instrument, i.e. as the quantity Δt, instead of as the simple reading of the clock.

The quantity Δt was chosen because a mass of experimental evidence shows that it is what represents time in the electromagnetic equations. We have no reason—at present, at any rate—to suppose that those equations do not represent the facts of electromagnetism, and we are therefore perfectly free to retain them so long as we understand what their symbols mean in terms of our measurements. What we can now see to be Einstein's error was to suppose that this quantity, Δt, was the actual reading which a clock would record at the arrival of a body which left a point at a distance Δx at zero time. That led him to force on kinematics a most inconvenient form of expression, that necessitated a ' contraction ' of rods, a ' slowing down ' of clocks, and an ' increase ' of mass when bodies were ' moving ', notwithstanding that ' moving ' was an attribute that could be applied or withdrawn by an act of will. These purely conceptual changes all vanish if the kinematical laws are expressed in terms of $\triangle s$ instead of $\triangle t$, and we recover the simple Newtonian expressions for the relations between mass, time and space measurements, in which the symbols represent the indications of instruments without modification. The importance of $\triangle s$ (though not its direct significance) was discovered by Minkowski a few years after Einstein's first paper was published. What he did, as we can now see, was to rediscover simple clock time, hidden in the hybrid $\triangle t$. Thinking, however, that $\triangle t$ was itself simple clock time, he believed he had discovered some objective thing, which has been called " space-time ", which is more fundamental than either space or time. That inversion of the actual situation has persisted in physics ever since.

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[1] See the author's paper, ' A Theory of Measurement ', this *Journal*, 1950, 1, 5.

Simplicity can be restored by ignoring $\triangle t$ for kinematical purposes, and retaining it, if at all, only for the expression of electromagnetic phenomena. Whether it will be worth while to reform the electromagnetic equations remains to be seen. In their present form, though without foundation in physical conceptions, they are nevertheless not too inconvenient in application, and it may well be that $\triangle t$ affords a simpler means of expression of the same facts than does $\triangle s$. But it is not profitable to speculate concerning this until we know by actual experiment how the velocity of light is related to that of its source.

It is, however, a legitimate question in the philosophy of science whether the representation of motion by any kind of space and time measurement is ultimately capable of expressing all the phenomena. There are grounds for suspecting that it is not. To begin with, the quantity, *velocity*, defined by the ratio of a space interval to a time interval, has meaning only in relation to a finite time interval. At one instant a body is at (x, t); at the next instant it is at $(x + \triangle x, t + \triangle t)$; and we must wait a time $\triangle t$ in order to know whether $\triangle x$ is zero or not. But in the case of motion in the line of sight (and if there is motion we can always suppose an observer stationed along its line) a single, ideally instantaneous, spectrum observation will show whether there is motion or not. Hence it is possible to choose a criterion—the Doppler effect—that has a greater scope for representing motion than has ' velocity ', and it may well be that in the last refinements this greater scope will be necessary for the full expression of all the phenomena presented by motion.

But there is a more general consideration pointing in the same direction. Motion is a *relation*, and a relation has a significance over and above anything that can be expressed in terms of the relata taken separately. We know very well how the Aristotelian logic, being limited to subject-predicate propositions, is impotent to deal completely with relations. We must therefore be prepared to discover that an expression of motion which is entirely dependent on the division of the phenomenon into the separate behaviour of two bodies distinguished by their location in space, may find that some aspects of motion are beyond its power to describe. This stage, indeed, we have already reached in the Einstein conception, in which both space and time locations are relative. That conception limits us to *point-events* as the ' atomic ' terms for the expression of phenomena. Consider two bodies, A and B, which move apart and then move together again. We may certainly hold that there is an event which we describe as that of the reversal of motion from recession to approach, but Einstein's theory does not allow its existence to be acknowledged. There is the event of B attaining its maximum distance from A, and the event of A attaining its maximum distance from B, but these are two different events, occurring at different places and at different times in the coordinate system in which either is at rest. There is no event of the reversal of motion. The ballistic theory, with its universal time, does not suffer from this disability; it can contemplate events covering a finite region of space; and since we can always choose to express our observations in terms of $\triangle s$, whatever experiment may show be to its relation to the readings of standard clocks, we may expect this expression to outlive that which employs $\triangle t$.

It would be of considerable philosophical interest to express the phenomena of motion in the language of the calculus of relations. So far as I know, this has never been attempted. For practical purposes, since all our observations must be made in relation to ourselves, it may well be that we shall always find an expression, in which our unique position is acknowledged, to be the one most directly related to experience, but the most comprehensive description of nature may nevertheless be possible only in purely relational terms. In any case it is unsatisfactory that, as in Einstein's theory, the concepts chosen to facilitate the study of the experience of motion should assume control and impose limitations on what they were intended merely to describe.

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GILBERT AND THE HISTORIANS (II) *

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IN the first part of this article I tried to show how some historical interpretations have distorted Gilbert's work because of their implicit inductivist assumption that it is possible to distinguish between bare experimental facts which are indubitable, and theories and speculations which are put forward to explain the facts. I suggested that Popper's thesis, namely, that there are no privileged observation statements upon which scientific theory can be inductively based, is amply illustrated by the details of Gilbert's work. It is therefore unprofitable to contrast, as many historians have attempted to do, Gilbert the experimenter with Gilbert the speculative theorist, for Gilbert's theories determine throughout the interpretations he makes of his experiments.

In considering Gilbert's theories and speculations, however, another interesting question arises, connected with the second main point of Popper's thesis. Where Gilbert explicitly puts forward theoretical explanations, is it possible to distinguish between those of his theories which are genuinely falsifiable, and those which are metaphysical in the sense of Popper's falsifiability criterion? There are two reasons for asking this question. Firstly, it leads to an assessment of Gilbert's theories on essentially different grounds from those of the inductivist historians, and also, secondly, it throws some light on the historical importance of the criterion itself, for it forms part of a general inquiry into the extent to which acceptance of the criterion *constitutes* the seventeenth-century revolution.

13 *Gilbert's magnetic forms*

I shall not deal here with Gilbert's cosmological speculations or their relation to the Copernican system, but shall concentrate on those of his theoretical explanations which might be expected to come closest to the subsequent practice of science, namely those in electricity and magnetism where his experimental work is strongest. This is not to say that his cosmology was unimportant, but it does not show so clearly the character of his ' new sort of philosophizing ' based on the experimental method. Gilbert himself makes this point, for he remarks that in Book VI (on cosmology), it is permitted to ' philosophize freely ' (Preface to the Reader).

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* The first part of this paper appeared in the previous Number.

Gilbert appears to be as confident as Bacon in the ability of experiments to demonstrate true causes:

> But after the magnetic nature shall have been disclosed by the discourse that is to follow, and perfected by our labours and experiments, then will the hidden and abstruse causes of so great an effect stand out, sure, proven, displayed and demonstrated; and at the same time all darkness will disappear, and all error will be torn up by the roots and will lie unheeded; and the foundations of a grand magnetic philosophy which have been laid will appear anew, so that high intellects may be no further mocked by idle opinions. (7)[1]

It is noticeable, however, that he makes this claim only in the case of his theory of magnetism, and indeed his theory of electricity is much less remarkable. The single chapter on electric attraction to which I have already alluded is a model of experimental investigation of traditional theories, but Gilbert's own theory in this case is hardly different in kind from those he has refuted, and not much more capable of development. He has no difficulty in disposing of some of the explanations of electric and magnetic attraction suggested by the Greeks and his Renaissance predecessors, such as that similars attract, and the attraction is by heat (as in evaporation), or by suction of a vacuum, or by air motions; but in the case of electricity, he considers, as we have seen, that the electric body is alone active and does not change the nature of the bodies which are drawn, and he finds that the attractive effect is screened off by dense media. He therefore concludes that the effect is due to some kind of material emanation from the electric. He suggests an analogy to explain its mode of action. Electric bodies are of a watery rather than an earthy nature, and the humour which they emit has a tendency to union and continuity like the cohesive tendency of water drops, and so it allures light bodies towards itself. Magnetic attraction on the other hand is not affected by the intervening medium:

> But if indeed these things resulted from a material ingression, then if strong and dense and thick substances had been interposed between the

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[1] Page references in brackets are to *On the Magnet*, ed. D. J. Price, New York, 1958.

> bodies, or if magnetical substances had been inclosed in the centres of the most solid and the densest bodies, the iron particles would not have suffered anything from the lodestone. But none the less they strive to come together and are changed [i.e. magnetised]. Therefore there is no such conception and origin of the magnetic powers. (66)

This conclusion leads Gilbert to the theory of magnetic forms which has been described pejoratively as metaphysical and animistic. Before considering this assessment we must discuss what Gilbert means by ' form '.

In the early part of the book, forms appear to be Aristotelian: they are qualities of bodies or properties of a species, with the added assumption that bodies having the same form, as iron and lodestone, will ' conform ' to each other, or affect each other in regular ways. But in distinguishing electric from magnetic effects Gilbert says:

> In all bodies in the world two causes or principles have been laid down, from which the bodies themselves were produced, matter and form. Electrical motions become strong from matter, but magnetic from form chiefly; and they differ widely from one another and turn out unlike, since the one is ennobled by numerous virtues and is prepotent, the other is ignoble and of less potency. (52)

The magnetic form referred to here is not a formal cause in the Aristotelian sense, but an efficient cause of magnetic motions (64). Magnetic bodies attract by ' formal efficiencies (*formalibus efficientiis*), or rather by primary forces (*vigoribus*) ':

> This form is unique and peculiar, it is not what the Peripatetics call the *causa formalis*, . . . but it is the form of the primary and original globes . . . not Aristotle's primary form, but that unique form which preserves and disposes its own globe. (65)

But apart from the fact that Gilbert regards this particular form as primary and prepotent, it is not clear that he intends anything more by it than the properties or forces of magnets as observed in their motions. He thought there were five such motions: (1) coition, (2) rotation towards the poles of the earth and direction of the earth towards the poles of the universe, (3) variation of the compass from the poles of the earth's rotation, (4) declination or dip of the magnetic needle, and (5) rotation of the earth which (Gilbert thinks) is natural to all spherical magnets. All these motions are said to be caused by ' concord ' or ' conformation ' of magnetic bodies with the earth and with one another, and there are many passages in which there is no need to read more into this language than Gilbert's assertion that he has discovered five distinct ways in which magnets act upon one another and described them accurately. Thus, his main theoretical assertion, as we have seen, is that the *cause* of the behaviour of magnetic needles on the surface of the earth is that the earth is a great magnet—the efficient cause or active agent is the earth, and does not lie (as had been suggested) in the sky, and the earth acts in this way because it has a form of a primary kind. When Gilbert discusses declination, he remarks that he has established the rules (*rationes*) of this motion in many striking experiments and that ' in the following pages we shall demonstrate the causes of it, in such a way that no sound, logical mind can ever rightly set at nought or disprove our chief magnetic principles '. (184). In the subsequent pages the '*causa certa et verissima*' turns out to be the conformation of the magnet to the earth (not its attraction to the earth's nearest pole), and the fact that this is an action of the whole earth (not of the earth's centre only) (187). And a diagram showing the angle of dip of needles at various points around a terrella is described in these words:

> While some assign occult and hidden virtues of substances, others a property of matter, as the causes of the wonderful magnetic effects . . . we have laid hold of the true efficient cause, as from many other demonstrations, so also from this most certain diagram of magnetic forces effused by the form. Though this form has not been brought under any of our senses, and on that account is the less perceived by the intellect, it now appears manifest and conspicuous even to the eyes through this essential activity which proceeds from it as light from a lamp (207).

The fact that the magnet acts at a distance shows that the form is ' effused ' beyond the limits of the body, but Gilbert insists that he does not mean by this that it is self-existent in space:

> And yet we do not mean that the magnetic forms and orbs exist in air or water or in any medium that is not magnetical, . . . for the forms are only effused and really subsist when magnetic substances are there (205).

In this the magnetic form is like light, which ' does not remain in the air . . . and is not reflected from those spaces ', but only exhibits its action when reflected from objects (77). Gilbert's analogy with light is instructive, for it shows that he does not accept the Aristotelian and medieval conception of propagation of form through a passive material medium as an explanation of the perception of bodies at a distance or of the action of the magnet. According to the theory developed by Roger Bacon and other medieval writers, images, forms or ' species ' of objects are ' multiplied ' in space under the influence of illumination, and the form which is thus conveyed across space becomes embodied in the retina of the percipient. In a similar way the magnet ' multiplies its species ' in space, and if there is iron in the vicinity, the species is actualised in it, causing it to become a magnet. In these theories there is no action at a distance, and the form is propagated by successively modifying portions of the material medium, as the impression of a signet ring modifies the wax. Gilbert, on the other hand, appears to accept a true action at a distance both in the case of reflected light and of magnetism. Just as the species of things appear in the eye only when both a luminous body and a reflecting object are present, he says, so the magnetic virtue appears only when two magnets are present. This interpretation of Gilbert's statements receives confirmation from the fact that, unlike Aristotle, he believes that there is void space in the heavens[1] and also that the heavenly bodies act upon one another magnetically; it therefore follows that in Gilbert's view magnetic virtue is not dependent on a medium for its propagation. ' It is in bodies themselves that acting force resides, not in spaces or intervals ' (217).

14 *Cause and Explanation*

So far, Gilbert's statements about the magnetic form are of two kinds. First, the form is the cause of the observable magnetic motions in the sense that it only subsists when they are present, and it is made ' manifest and conspicuous ' by them. But it is not identical with them, because a quality of primacy and prepotency is ascribed to it which is metaphysical in the sense that it does not appear to add anything to the empirical content of the form. If this is the case Gilbert has not, in the modern sense, put forward any causal *explanation* of magnetic effects by speaking of a magnetic form, and yet in some sense he does regard it as a cause.

In order to understand this sense, it is necessary to distinguish the widely accepted modern view of explanation as construction of a theory from which the phenomena to be explained and new observable phenomena are deduced, from the view of explanation as a ' making plain ' by describing an unusual or unexpected or otherwise puzzling event in terms of events more familiar or more intelligible. The second kind of explanation is not sufficient for the first, and may not even be necessary, although explanation in terms of familiar models is one important way in which explanations of the first kind have been realised.

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[1] *Philosophia Nova*, Amsterdam, 1651, p. 30

Gilbert does not use even his theory of electric attraction as an explanation of the first kind. He makes no deductions from the theory of electric humours which could be tested or lead to new discoveries, and when he comes across an apparent refutation, he modifies the theory so as to accommodate the awkward fact. For example, he refutes the assertion that electric attraction is due to the motion of air by bringing a flame up to a piece of attracting amber and showing that the flame is not disturbed. But lest it be thought that the electric effluvium which he postulates would also disturb the flame, he says that this is ' much more subtle than air ' (55). Again, he shows that moisture in the atmosphere *hinders* attraction, how then can it be that a moist humour causes it? An *ad hoc* and somewhat obscure explanation is given by appealing to the behaviour of sticks partly immersed in water (56-58). The treatment of magnetic ' conformation ' is equally *ad hoc*. In so far as this notion arouses any expectations about the behaviour of magnets over and above an actual description of their known motions, these expectations are surely refuted by the following experiment which Gilbert reports as something of a paradox. Suppose one end of an iron rod is touched by the north pole of a terrella and the rod then removed to a short distance. It will tend to swing with the end which has been touched pointing to the north pole of the terrella. But if a piece of the terrella itself in the shape of a rod be cut out along a magnetic meridian, and this piece removed to a short distance, it will not remain in its original orientation, as one might expect if pieces of a body ' conform ' to their parent body, but will swing round to point in the opposite direction. But this is of course perfectly consistent with the rules about coition of unlike poles, and Gilbert remarks that these rules show the ' true and genuine conformation ', for ' magnetic substances seek a unity as regards form; they do not so much respect their own mass ' (122). In other words, they behave according to the rules described, and this *is* to ' conform '. The notion of conformation does not constitute a testable theory in the modern sense.

In the sense of making comparison with a more familiar process, however, Gilbert's explanation of electric attraction clearly qualifies. If one is prepared to accept the self-attractive properties of moisture as 'natural' or for some reason not demanding further explanation, and if it can be shown that an electric humour having the same properties would account for all known phenomena of electric bodies, then it may be said that electric attraction itself is explained. It was this conception of explanation of the unfamiliar in terms of the familiar that was attacked by Bacon with the comment that the familiar and ordinary must also be explained, but he so far agreed with the process exemplified by Gilbert's electric humour as to suggest that the ultimate explanation is often better sought in the ordinary than in the odd—the nature of cohesion for instance in solid bodies rather than in soap bubbles.[1]

Is Gilbert's magnetic form an explanation even in this sense? What familiar process has sufficient analogy with magnetic actions to qualify as an explanation of them? Gilbert has already refuted the analogies with vacuum suction, attraction by heat, propulsion by circular air currents, and action of humid emanations, in the case of magnetism, and has decided that nothing material passes between magnets to cause their mutual actions. No action, he thinks, takes place *by means of matter* except by contact (57). But this action does not take place by means of matter: it may therefore either be an action at a distance, or a manifestation of self-movement requiring no external propulsion. What is it that manifests self-movement? Clearly the answer is that *animated* bodies do so, and so Gilbert arrives at his analogy between the magnet and self-moving souls—his so-called animism. The magnet can transmit its form to a distance without a material medium. That is a pure action-at-a-distance, and requires, Gilbert thinks, a soul of higher order even than our own (210). The form itself which is transmitted is a power of self-movement, showing itself when two magnets 'mutually agree' to conform to one another. The magnetic force

> seems to be very like a soul. For the power of moving itself seems to point to a soul; and the supernal bodies . . . are thought by some to be animated, because they move with admirable order (68).

Here we have the positive analogy between the magnet and the soul—the power of self-movement, and movement according to the order and harmony of the whole system. This movement tends to preserve the heavenly bodies 'for their own good', so that the universe is prevented from falling into 'wretchedest chaos' and the earth from being 'vacant, dead and useless' (210, 227). After full allowance has been made for Gilbert's teleological and animistic language here, it should be noticed that the chapter in which he introduces this discussion of the magnetic soul is headed 'Magnetic force is animate, or *imitates* a soul (*anima*); and in many things surpasses the human soul while this is bound up in the organic body' (208, my italics). Gilbert goes on to point out the *negative* analogy between the magnet and the soul, and although he uses this for the quasi-theological purpose of showing how much more noble are the heavenly bodies (including the earth) than human souls, nevertheless in the light of these passages he cannot be accused of making a facile identification of the heavenly forms and human souls. The negative analogy is as follows: the globes are not restricted by possession of organs, they are incorruptible and perfect, they are able to emit 'immaterial effused forms' outside the limits of their bodies, all of which distinguish them from human souls which are 'brought and carried away by a breath',[1] and finally, the globes make no mistakes in their rational and orderly movements:

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[1] Bacon, *Novum Organum*, Book I, lxxxviii

> But those motions in the sources of nature are not caused by thinking, by petty syllogisms, and theories, as human actions, which are wavering, imperfect, and undecided; but along with them reason, instruction, knowledge, discrimination have their origin, from which definite and determined actions arise, from the very foundations that have been laid and the very beginnings of the universe (210).

What, then, can we conclude about the status of Gilbert's 'true causes' of magnetism? First, that the magnetic form is something that manifests itself in five kinds of motion observably exhibited by magnets. Second, that the heavenly bodies are magnets in the sense of exhibiting these motions. This is a theory in principle capable of test, for the orbits of the heavenly bodies can be compared with the behaviour of experimental terrellae. But if we ask, thirdly, how the magnetic form is a theory of the *nature* of magnetism, or a *cause* of magnetism as opposed to a summary description of magnetic behaviour, it must be said that it gives a causal explanation only in the sense of giving a familiar analogy, and not in the sense of being a falsifiable theory. The form is the cause of magnetic motions in the way that the soul is the cause of animal motions, but there is no pretence that this analogy can be pressed further to admit of tests and new predictions. It is metaphysical in the sense of being unfalsifiable.

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[1] Mottelay's translation of *De Magnete* is so inaccurate here as to have led at least one commentator into ascribing to Gilbert the assertion that the *immaterial effused forms* of magnets are 'brought and carried away by a breath', that is, that magnetic motions are caused by an effluvium which, because more subtle, is more 'immaterial' than matter. But when Gilbert says 'immaterial', he means 'not material'.

15 *Gilbert and Kepler*

It has been claimed that Gilbert's metaphysical magnetic form initiated the idea of a field of force, but this cannot be said to be accurate, for Gilbert would certainly have rejected a statement such as Faraday's to the effect that lines of magnetic force exist as a ' condition of space free from . . . material particles '.[1] Gilbert was rather the first to discuss in detail a true action-at-a-distance, and to use the Aristotelian distinction of matter and form to help him to define it. But like many theories which with positivist purity try to avoid the assumption of unobservable interphenomena, this one led to no theoretical developments in the way Gilbert stated it. It only did so by being, perhaps deliberately, misunderstood. The misunderstanding, if it were such, was Kepler's. In his *Mysterium Cosmographicum* of 1596, Kepler is already comparing terrestrial gravity with magnetism (an identification which Gilbert never made),[2] and speaking of an *anima motrix* in the sun which causes the motions of the planets.[3] Later (probably encouraged by *De Magnete*), he regards this latter force as magnetic in nature. He ascribes to Gilbert the doctrine that ' magnetic fibres or filaments ' extend from the south to the north poles of the earth,[1] and develops this in his own theory that the sun directs the planets by means of ' filaments or fibres ' emitted in the zodiacal plane. But Kepler's force, unlike Gilbert's, exists in space as a ' species ',[2] and its mode of action is nearer to the medieval ' multiplication of species ' than to Gilbert's carefully defined magnetic form. And finally Kepler drops the word ' anima ' and replaces it by ' vis ' for a reason which shows that his conception of ' anima ' is very different from that of Gilbert. He discovers that the velocity of a planet varies inversely with its distance from the sun, and this precise quantitative variation leads him to the conclusion that what is emitted from the sun must be corporeal ' at least in a certain sense '.[3] Variation according to precise rule would never have led Gilbert to drop his notion of the ordering soul of the sun, for it was just this property of regularity which had suggested it to him.[4]

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[1] Faraday, *Experimental Researches*, Vol. III, p. 414

[2] Gilbert is still widely misinterpreted on this point. The notion that he ascribed gravity to magnetism seems to go back to Bacon, who remarks (*Novum Organum* II, xxxv) ' . . . if Gilbert's opinion be received, that the earth's magnetic power of attracting heavy bodies does not extend beyond the orb of its virtue (which acts always to a certain distance and no more) . . . ' *Cf.* also his fragmentary introduction to *The History of Heavy and Light Works* (ed. Ellis & Spedding, Vol. V, p. 202). Bacon has confused Gilbert's magnetic ' orb of virtue ' (xvj) with his ' orb of effluvia ' (229)(the atmosphere), which causes heavy bodies to descend to their parent body, the earth, and which unlike magnetic force, acts *by contact*. Hooke, following Bacon, misinterprets Gilbert in the same way: ' Gilbert began to imagine it [gravity] a magnetical attractive power ' (a paper of 1666 reported in Birch: *History of the Royal Society*, London, 1756, Vol. II, p. 70).

[3] Kepler, *Opera*, ed. Frisch, Vol. I, p. 174

It must be remembered that the connotations of ' body ', ' spirit ', and ' soul ' were very various in Renaissance writers and not clearly distinguished from each other. Distinction and clarity were to some extent introduced by the Cartesian dualism and the dogmatic mechanism of subsequent physics, with its ' spiritual effluvia ', which were material. It is to Gilbert's credit that he is not trapped into mechanism as an explanation of magnetic phenomena, as were all his seventeenth-century successors until Newton, and yet at the same time he manages to avoid the vagueness of his predecessors. It is true, as Koyré remarks, that Gilbert's magnetic form could not be mathematised[5] and so played little part in the developments which culminated in Newton's *Principia Mathematica*, but on the other hand the diagrammatic and numerical character of Gilbert's work places it firmly in the Platonic mathematical tradition.

16 *Gilbert and Bacon*

Finally, it is of interest to compare the roles of Gilbert and Bacon as initiators of the experimental philosophy. It has often been remarked as a puzzle [1] that although Gilbert agrees with Bacon that experiments are the true foundations of natural philosophy, Bacon does not seem over-anxious to claim him for an ally, sometimes conclusions detrimental to Bacon's character have even been drawn from this fact. But a dispassionate reading of Bacon lends no colour to the picture of him as a jealous and vindictive ignoramus, deliberately disparaging Gilbert's views and suppressing his work.[2] It is always preferable to find reasons based on philosophic grounds for philosophic disagreement rather than those based on personalities, and whatever may be the truth about the latter, Bacon himself gives clear indications of the philosophical reasons for his critical remarks about Gilbert's method. Apart from these criticisms, almost all his comments on Gilbert's results and opinions are either favourable or else polite expressions of dissent from Gilbert's astronomical views, for Bacon is anti-Copernican, and although his opposition here may reflect his ignorance of current astronomy, it surely cannot be ascribed to any particular antipathy to Gilbert.

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[1] Kepler, *Werke*, ed. Caspar, Vol. VII, p. 334

[2] Kepler, *De Stella Martis* (1615), *Werke*, Vol. III, pp. 246, 350

[3] Kepler, Notes (1621) to *Mysterium Cosmographicum*, *Opera*, Vol. I, p. 176

[4] Dr Agassi has rightly pointed out that in endowing magnets with souls Gilbert merely endowed them with self-movement and harmony (this *Journal*, 1958, **9**, 240). Kepler's earlier view is more properly called ' animism ' in the sense of the sixteenth-century cosmologies (*cf.* R. G. Collingwood, *Idea of Nature*, Oxford, 1945, pp. 94 f.).

[5] Koyré, *Etudes Galiléennes*, pp. 99, 138, 148

The passages which are relevant to Bacon's views of Gilbert's method are those in which he inveighs against the Empirics and the philosophers who, seduced by the Idols of the Cave (that is, their own predilections), build a whole philosophy out of the results of a few experiments:

> The race of chemists again out of a few experiments of the furnace have built up a fantastic philosophy, framed with reference to a few things; and Gilbert also, after he had employed himself most laboriously in the study and observation of the lodestone, proceeded at once to construct an entire system in accordance with his favourite subject.[3]

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[1] Most recently by D. H. D. Roller: *Isis*, 1953, 44, 10

[2] *Cf.* J. Pelseneer: 'Gilbert, Bacon, Galilée, Kepler, Harvey et Descartes', *Isis*, 1932, 17, 171; Benjamin: *Intellectual Rise in Electricity*, pp. 317 f. The accusation of suppression made by Benjamin is based on the fact that the manuscript of *Philosophia Nova* was in Bacon's possession at his death, and that there are references to Gilbert's views expressed therein in Bacon's writings. But it does not follow that Bacon had any evil motives in not causing them to be published during his lifetime. It is at least as likely that he thought their publication would *damage* Gilbert's reputation (as has in fact been the case among Bacon's disciples, the inductivist historians), or at least that they do not add anything important to *De Magnete* (which has also been the subsequent general opinion).

[3] Bacon, *Novum Organum*, Book I, liv. Cf. ibid. lxiv; *Advancement of Learning*, *Works* ed. Ellis & Spedding, Vol. III, 293. Other references to Gilbert are to be found in *Works*, Vol. III, 366; Vol. IV, 179, 218, 220, 223, 323, 360, 426; Vol. V, 202, 454, 493, 515, 518, 533, 537, 556.

The charge is, then, that Gilbert, like the alchemists and other Empirics, performs multitudes of experiments without order and method, and then leaps to the unjustified conclusion that their results provide him with universal explanations. Since a commonly accepted view of Baconian science is precisely that it falls itself under the first part of this indictment, it is necessary first to remark that Bacon's method does not depend on any such indiscriminate gathering of facts, nor does it stop at the level of experiments, but envisages the construction of elaborate theories. His method of drawing up Tables of Presence and Absence involves selective observation and artificial experiment which is already far from the unsystematic procedure of which Bacon accuses the Empirics. But this is not the end of the matter—Bacon also has a vision of a 'Ladder of Axioms' up which the investigator may climb from phenomena to more and more general Forms, and at any stage of which he may descend to new observations.[1] It is therefore essential to Bacon's method that explanations should enable new predictions to be made,[2] and here Bacon's precept was undoubtedly better than Gilbert's practice. Bacon's problem in looking for Forms was a problem of fundamental physics (what he called 'metaphysics'), and in the case of magnetism it would have been the problem of the 'nature' of the phenomenon, expressed in terms of configurations of ultimate, general, elements of the world, and enabling magnetism to be linked, via the Ladder of Axioms, with other phenomena, known and yet to be discovered. In so far as Gilbert's theory of magnetism was either merely descriptive, or metaphysical in the sense of yielding no predictions, he fails to provide what Bacon demands,[3] and what the subsequent history of electromagnetism has in fact provided. In other respects, however, the subsequent practice of science justifies Gilbert rather than Bacon. Where Gilbert is looking for the 'Form' (in the Baconian sense) of electric attraction, he uses a method of crucial experiment and elimination which is similar to Bacon's method, but what Gilbert eliminates are previous *theories*, not, as Bacon does, *qualities* which lie on the surface of phenomena and are immediately apparent to the empty mind. And Gilbert makes no attempt to show that he has eliminated all but one of the possible theories, but merely replaces a few refuted theories by another which, according to Bacon, is equally an illegitimate anticipation. Most of Gilbert's experiments do not have the form of construction of tables of presence and absence of qualities, but are designed and described in accordance with his theories, in order to develop and test his theories, and most of these theories are in the Baconian sense the wildest anticipations. Given Bacon's standpoint, his comments on Gilbert are quite intelligible, but Gilbert's hypothetical method is nearer the pattern of later physics, for the empty mind is an illusion.

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[1] For an analysis of Bacon's philosophy of science see my 'Francis Bacon', forthcoming in *A Critical History of Western Philosophy*, ed. D. J. O'Connor.

[2] See especially Bacon, *Novum Organum*, Book, I, cvi.

[3] Bacon does not explicitly charge Gilbert with this last offence, but it is implied in his comparisons of Gilbert with the alchemists, for of them he says that they only make new discoveries by accident, whereas Bacon's method will lead to new and useful results as consequences of the discovery of true axioms.

Whipple Museum, Cambridge

DISCUSSIONS

THE FALSIFIABILITY OF THE LORENTZ-FITZGERALD CONTRACTION HYPOTHESIS: A REJOINDER TO PROFESSOR DINGLE

PROFESSOR Dingle's reply [1] to my refutation [2] of Popper's *ad hoc* charge against the original Lorentz-Fitzgerald contraction hypothesis makes the following three claims:

(i) 'The [Lorentz-Fitzgerald] hypothesis was certainly *ad hoc*, for the *realisation* of its falsifiability came very much later, after it had been displaced by the special relativity theory of Einstein.' And my demonstration that the Lorentz-Fitzgerald contraction hypothesis is *not ad hoc* within the framework of the aether theory is thus indicted as being 'much more misleading than the original statement [by Popper]'.

(ii) When, in 1903, Lorentz deduced his *pre*-relativistic transformations from two specified fundamental assumptions and then, in turn, derived the Lorentz-Fitzgerald contraction from his transformations, the contraction 'ceased to be an *ad hoc* hypothesis, and became falsifiable by anything that would falsify these two assumptions'.

(iii) Concerning the availability of experimental evidence that might have falsified but in fact confirmed special relativity and, in particular, Einstein's assumption that the velocity of light in space is independent of the state of motion of the emittor, Dingle says the following: 'The . . . assumption . . . could be made the subject of a laboratory experiment to compare the times of arrival of pulses of light, proceeding from bodies in relative motion but emitting at the same point, at a distant point along the line of motion. This was, of course, impracticable in 1905, but it should not be beyond the power of modern techniques. It is highly desirable to test, if possible, this basic assumption of existing physical theory.'

The aim of the present note is to show that the first two of Dingle's assertions are untenable and that the statement of his plea for a laboratory test of Einstein's assumption is quite misleading as to the amount of observational evidence already available to support it.

(1) The following two very different questions are confounded by Dingle: (i) Is a certain auxiliary hypothesis independently testable, in principle, within the logical framework of the theory which it modifies?, and (ii) Is the proponent of the collateral hypothesis at issue *aware* of such independent testability, if it obtains? To assert that an auxiliary hypothesis is *ad hoc* within the context of the theory to which it pertains is to attribute a certain logical property to that hypothesis in the specified theoretical framework. Dingle overlooks that the possession of this logical property by the hypothesis cannot be held to consist in the failure of its advocates to *realise* that it does, in fact, lend itself to being tested. As well say that a certain proposition which is, in fact, provable in a given axiom system can be asserted *not* to be a theorem at a certain time on the strength of the failure of all mathematicians to *see* then that its proof is feasible.

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[1] This *Journal*, 1959, 10, 228

[2] This *Journal*, 1959, 10, 48

What Dingle does establish is the *historical* fact of Lorentz's personal methodological culpability, viz. that Lorentz espoused his contraction hypothesis in the face of his own belief that it was not susceptible to independent test. But, contrary to Dingle, this *biographical* fact does *not* show that the hypothesis was *ad hoc* prior to 1903. As I endeavoured to point out in my earlier note, the incorrectness of the *ad hoc* charge is tellingly demonstrated by the capability of the Kennedy-Thorndike experiment (and of others) to furnish an *independent test* of the Lorentz-Fitzgerald contraction hypothesis. To this Dingle replies that *awareness* of this capability did not come until well after the modified aether theory had been supplanted by Einstein's special theory of relativity. But this retort is quite irrelevant. For the refutation of the *ad hoc* charge by the Kennedy-Thorndike experiment is *internal to the aether theory* and no more depends logically on the availability of the special theory of relativity than does the refutation of the *original* aether theory by the null outcome of the Michelson-Morley experiment.

(2) Even if Dingle *were* right in maintaining that the contraction hypothesis was *ad hoc* prior to 1903, the reason given by him for the cessation of the alleged *ad hoc* status during that year does *not* entail this conclusion, as he supposes. He reaches the latter conclusion by inferring fallaciously that the hypothesis became falsifiable in 1903 in virtue of the falsifiability of the two fundamental assumptions from which Lorentz had deduced it during that year via his pre-relativistic transformation equations, a *non-sequitur* which denies the antecedent in a conditional argument.

(3) New, improved tests of a hypothesis are, of course, always a *desideratum*. But by calling for a laboratory test of Einstein's assumption that the speed of light is independent of the motion of the source *without* mentioning the already existing evidence for it, Dingle misleadingly suggests that to date this assumption is still devoid of an experimental foundation. Not only do we have corroborating evidence for it, but that evidence was sufficiently compelling to convince an investigator like R. C. Tolman,[1] who had been an exponent of the most natural rival of Einstein's assumption,[2] viz. of Ritz's version of the emission theory of light. According to that emission theory, the speed of light is c only relative to its source, while in any other frame K, the velocity of light and the velocity of its source are additive. And the observational findings *disconfirming* Ritz's alternative to Einstein's assumption include the following results, among others:[3]

(i) On Ritz's theory, the execution of the Michelson-Morley experiment with light coming *not* from a terrestrial source but from the sun should issue in a shift of the interference fringes on rotation of the apparatus. But Tomaschek reported in 1924 that he had found no such effect, and

(ii) In experiments involving *open* rather than closed light paths, the Einstein and Ritz assumptions entail differences even in regard to effects of only the first order in the quantity v/c. To be sure, no open light path experiment of the type suggested by Dingle seems to have been performed with *terrestrial* sources of light. But this fact can hardly detract from the force of the *astronomical* evidence pertaining to first order effects which Comstock and de Sitter have marshalled against the Ritz hypothesis : the existence of distant double stars whose orbits are observed to have *small* eccentricities strongly disconfirms Ritz's postulate by showing that the speed of light could differ from *c* by at most only a small fraction of the velocity of the source.

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[1] Cf. R. C. Tolman, *Relativity, Thermodynamics and Cosmology*, Oxford, 1934, pp. 15-17

[2] Cf. R. C. Tolman, *Physical Review*, 1910, **30**, 291 and 1910, **31**, 26

[3] For a comprehensive account, see W. Pauli, *Theory of Relativity*, London, 1958, Part I, §3, pp. 6-9 and 207; also, the reference in footnote 1 above.

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REPLY TO PROFESSOR GRÜNBAUM

IF I were sure that I understood exactly what Professor Grünbaum is now saying, which I am not, I do not think I should consider that it justified protracted discussion. Nor, in fact, did I regard his original point—the question whether the Fitzgerald-Lorentz contraction hypothesis is falsifiable or not—as more than a matter of trivial detail, a question whether a particular event among many was or was not an example of a general principle. I wrote my comment because the matter seemed to me to have incidental aspects which are important at the present time, to which I thought it worth while to call attention.

But to save meanderings along unprofitable by-paths, I would now merely point out—as I should have done at the beginning if I had been discussing the matter for its own sake—that Professor Grünbaum's statement that the Kennedy-Thorndike experiment could falsify the contraction hypothesis is simply incorrect. The experiment gave a null result, which was consistent with the contraction and the ' time dilatation ' operating together. If a positive result had been obtained, then, depending on its amount, it could have been deduced that the contraction alone, or contraction supplemented by a different time dilatation, had been established. No conceivable result of the experiment could have falsified the contraction hypothesis, or even increased the probability of its falsity.

With regard to the experimental evidence for Einstein's postulate of constant light velocity, Professor Grünbaum has apparently not seen a paper in *Mon. Not. R.A.S.*, 1959, 119, 67. There is at present no experimental evidence at all either for or against this postulate.

HERBERT DINGLE

THE PARADOX OF CONFIRMATION *

HEMPEL's paradox of confirmation has recently been discussed by a number of authors[1] but I wish to offer a completely different treatment. The paradox is exemplified thus :

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* I wish to thank the referee for some incisive criticisms while this note was in course of preparation.

[1] References may be traced through J. Agassi, this *Journal*, 1959, **36**, 311-317.

The hypothesis, H, that all crows are black is the same as that all non-black things are not crows, and this is supported by the observation of a white shoe. Which seems paradoxical.

I shall argue that :

(i) The observation of a white shoe *does* support H (provided that the number of non-black objects that might be observed is known to be large compared with the number of crows), and the result appears paradoxical because the support given to the hypothesis is negligible. The argument depends on the judgment that a shoe can be regarded as a typical non-crow.

(ii) More rigorously, i.e. independently of the above kind of judgment, the observation of a non-black non-crow supports H if the observation is of ' stoogian type '. The concept of the Stooge, which will be defined below, is, in my opinion fundamental in statistical practice.

(iii) When not of stoogian type the observation of a non-black non-crow does not necessarily support H, and may even undermine it to some extent if the object observed bears a strong resemblance to a crow. For example, the observation of a white raven undermines H.

The main issue will be brought out by imagining first that the only objects we can observe are crows and shoes, and the only colours black and white. These assumptions will be relaxed later.

Imagine a 2 by 2 contingency table in which the rows are labelled ' crow ' and ' shoe ' and the columns ' black ' and ' white '. The hypothesis H states that one of the cells in this table is empty. To get an entry in the table in either of the two cells that are adjacent to the empty cell is support for the hypothesis that it is empty. If quantitative considerations are ignored then the problem is symmetrical in rows and columns, and a white shoe is as good as a black crow.

It is not strictly accurate to ignore quantitative considerations since the argument breaks down in some extreme cases. In order to make the argument quantitative it is convenient to make use of the terminology of ' odds ', of ' Bayes factors ', and of ' weights of evidence '. A weight of evidence is an explicatum of ' support ' for a hypothesis, and it was actually called ' support ' by Jeffreys.[1]

If p is a probability, the corresponding odds are $p/(1-p)$, for example, a probability of $\frac{1}{2}$ is odds of 1. Odds run from 0 to infinity when p runs from 0 to 1. If H is a hypothesis and E is evidence, then $P(H|E)$ denotes the probability of H given E, and the odds of H given E are

$$O(H|E) = P(H|E)/P(\overline{H}|E),$$

where $\overline{H}$ denotes the negation of H.

The initial probability of H is $P(H)$. Strictly there is always some initial evidence before an observation is made, but it is often convenient to take it for granted and to omit it from the notation. As a consequence of an observation that provides evidence E the final probability of H is $P(H \mid E)$.

The factor by which the initial odds are to be multiplied in order to get the final odds is $O(H \mid E)/O(H)$. It is called the ' Bayes factor ' in favour of H and is equal to

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[1] H. Jeffreys, ' Further Significance Tests ', *Proc. Cam. Phil. Soc.*, 1936, **32**, 416-445

$P(E \mid H)/P(E \mid \bar{H})$, as may easily be deduced from the product axiom. This ratio is a simple form of the 'likelihood ratio', but the expression 'likelihood ratio' is also used in a more general sense in which it is not equal to the Bayes factor.

A Bayes factor exhausts all the information from an experiment, or from evidence, that can be relevant to the probability of H. This statement is obvious if we are allowed to talk about the probabilities of hypotheses (and if these probabilities are not zero), since the factor determines the final probability completely in terms of the initial probability. Since this statement is true whatever the initial probability it is also true even if the initial probability is unknown. If however a language is adopted in which it is forbidden to talk about the probabilities of hypotheses, then the statement can still be justified in terms of the Neyman-Pearson theory of 'errors of the first and second kinds', provided of course that the simple likelihood ratio is used instead of the Bayes factor.

The discussion of the case where $P(H) = 0$, which is regarded as important by Popper, would take us too far afield.

Let the logarithm of a factor be denoted by $W(H : E)$, or more generally by $W(H : E \mid G)$ when the probabilities are conditional on another proposition, G. This 'log-factor' has the additive property.

$$W(H : E \,.\, F) = W(H : E) + W(H : F \mid E).$$

Therefore a log-factor does not merely tell us everything about the evidence that can be relevant to the probability of H; it also has an additive property analogous to that of ordinary weights in the scale of a weighing machine. (The analogy is even stronger when E and F are statistically independent given H and also given $\bar{H}$, since the last term then reduces to $W(H : F)$. This happens when both H and H are so-called 'simple statistical hypotheses'.) $W(H : E \mid G)$ is therefore appropriately called the 'weight of evidence', or, more fully, the weight of evidence in favour of H (against $\bar{H}$) provided by E given G. When we are taking for granted that one of two hypotheses is true, so that we may describe one as the negation of the other and denote them by H and $\bar{H}$, then the weight of evidence for H provided by E (if it can be calculated) gives a complete summary of the evidence from E. Nothing else about E can be relevant to the selection between H and $\bar{H}$. But the initial probability of H, and the utilities of H and $\bar{H}$ if true, are relevant also to the choice between them. The choice should be consistent with the 'principle of rational behaviour', which is the recommendation to maximise expected utility.[1] Since E tells us nothing about the initial probabilities and utilities, it follows that the only thing in E relevant to the choice is the weight of evidence. The unit in terms of which weights of evidence are measured depends on the base of the logarithms. (Cf. the decibel.)

Now suppose that there are N objects that might be seen at any moment, of which c are crows and b are black, and that the N objects each have probability $1/N$ of being seen. Let H_i be the hypothesis that there are i white crows, and suppose that the hypotheses $H_1, H_2, \ldots, H_c$ are initially equiprobable. Then, if we happen to see a black crow, the Bayes factor in favour of H is

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[1] See, for example, I. J. Good, *Probability and the Weighing of Evidence*, London, 1950, p. 40; 'Rational Decisions', *J. Roy. Statist. Soc., ser. B*, 1952, 14, 107-114; L. J. Savage, *The Foundations of Statistics*, New York and London, 1954.

$$\frac{c}{N} \div \text{average}\left(\frac{c-1}{N}, \frac{c-2}{N}, \ldots, \frac{1}{N}\right) = \frac{2c}{c-1},$$

i.e. about 2 if the number of crows in existence is known to be large. But the factor if we see a white shoe is only

$$\frac{N-b}{N} \div \text{average}\left(\frac{N-b-1}{N}, \frac{N-b-2}{N}, \ldots, \max\left(0, \frac{N-b-c}{N}\right)\right)$$
$$= (N-b) \div \max(N-b-\tfrac{1}{2}c-\tfrac{1}{2}, \tfrac{1}{2}(N-b-1)),$$

and this exceeds unity by only about $\frac{1}{2}c/(N-b)$ if $N-b$ is large compared with c. Thus the weight of evidence for H provided by the sight of a white shoe is positive, but is small if the number of crows is known to be small compared with the number of non-black objects.

Similar results can be obtained if the two assumptions of equiprobability are dropped, although the algebra is more complicated.

The argument can be generalised still further by allowing N, c, and b, themselves to have probability distributions, not necessarily known to us. The same conclusions will follow if N is finite, say less than the exponential of the number of particles in the observable universe. The qualitative results are distribution free if $N-b$ is assumed to be certainly (or even only highly probably) large compared with c; and they are independent of whether we use a theory of personal (subjective) probability or of ' credibility ' (logical probability).[1] If $N-b$ is not probably large compared with c the argument breaks down. For example, suppose that all objects ever seen in the past were black. Then the observation of a white shoe would undermine the hypothesis H.

In order to generalise from ' white shoes ' to ' non-black non-crows ', I introduce the Stooge. A stooge will be understood to be a man who must carry out an observation and then reply ' yes ' or ' no ' to the questions ' was it black ? ' and ' was it a crow ? ' If he gives any other information he will be shot dead and knows it. Thus an observation of stoogian type in effect lumps together white shoes, green pebbles, and horses of all colours except black ones. For stoogian observations the previous argument applies with the rows of the contingency table labelled ' crow ' and ' non-crow ', and the columns ' black ' and ' non-black '. Therefore a stoogian observation of a non-black non-crow supports H.

The use of a stoogian observation is equivalent to a decision to avoid certain types of information on grounds of simplicity and objectivity. An example is the decision to avert our gaze from the particular choice of random numbers in a randomised statistical design, in order that we should be able to make an objective statement of significance. Another example is the decision in law to ignore the evidence of a copy of a copy. The verbal simplification of evidence by ignoring some of it is indeed so familiar in ordinary life that it becomes a habit. Whenever we communicate our observations to others we have continually to judge what it is fair to omit. Omission is usually a lesser distortion than insertion, and in stoogian observations the decision what to omit is made in advance, so that personal bias is minimised. (Perhaps this is the justification of those forms of cross-examination in which the person being interrogated is expected to answer the question asked, without much elaboration.)

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[1] For a recent article on kinds of probability see I. J. Good, *Science*, 1959, **129**, 443-447.

The purpose of introducing stooges in the present discussion is much the same in other contexts, namely that it simplifies the theory. We can first prove our thes rigorously under the limitation that the observation is stoogian. Having done th we must go on to consider what modifications are required if the observation is n stoogian.

The paradox can be stated in the form " the proposition 'I have just seen a nor black non-crow' supports *H* ". Although this form is true it does not follow th if someone correctly says " I have just seen a non-black non-crow " that *H* is sup ported, for he may be maliciously suppressing some relevant information. But whe a Stooge expresses a proposition by means of a remark, the fact that he has made th remark conveys the same information about *H* as does the proposition expresse For Stooges do not *maliciously* suppress information.

If, after a stoogian observation is made, we happen to acquire the suppresse information, we have to judge whether this information should cause us to revise ou conclusions. (For example, we might discover that all our random numbers wer zeros.) A white shoe seems to me to be a typical non-black non-crow, so that i this case it makes little difference whether the observation is stoogian. But a rave is not a typical example of a non-crow, and it is intuitively obvious that the non stoogian observation of a white raven would undermine the hypothesis that all crow are black.

The argument concerning the white raven can also be made quantitative by think ing in terms of a contingency table, this time 2 by 3. The columns may be labelle 'black' and 'white' as before. The rows may be labelled 'crows', 'ravens', an 'other things'. We may denote by $H_{i,j}$ the hypothesis that there are i white crow and j white ravens. Any reasonable initial probability distribution for the $H_{i,j}$' should incorporate the assumption that i and j are correlated, since crows and raven belong to the same family of birds. Under a wide class of reasonable assumptions i will be found that the observation of a white raven undermines the hypothesis that al crows are black.

For a stoogian observation it is unnecessary to think in terms of a contingenc table of dimensions more than 2 by 2.

The justification for the three assertions listed at the beginning of this note nov seems to me to be adequate.

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ERRATA

In the May No., 1960, p. iii, 'Mr B. Braithwaite' should, of course, have beer 'Professor R. B. Braithwaite'.

In the errata of the same No., p. 88, a mistake has been made. The followin appeared: "p. 315 insert, on the left hand side of the table of possibilities at th bottom of the page, '$\omega =$' before '$\sim$ e', so that '$\omega =$ e' stands before the lowes bracket." Of course the end of the sentence should have read: "'$\omega = \sim$ e' stand before the lowest bracket."

REVIEWS

Towards a Unified Cosmology. By Reginald O. Kapp.
Hutchison & Co., London, 1960. Pp. 303. 35s.

Scientists and philosophers often find it difficult to agree as to what constitutes the ' philosophy of science ' ; and indeed it has been said that this subject is frequently neither good philosophy nor good science. Such a criticism could not be levelled at this book by Professor Kapp. *Towards a Unified Cosmology,* as its title implies, is concerned to develop a general theory of Cosmology and to examine its consequences; and the general theory is arrived at by the rigorous application, to possible hypotheses of the past and future extension in time of the material universe, of what is called ' The Hypothesis of Minimum Assumption '.

The Hypothesis of Minimum Assumption is, in effect, Ockham's Razor, or the rule of economy of hypotheses; and it is Professor Kapp's contention that this is, in fact, the most basic of all the principles of physics. According to him the laws of physics are not restrictive in the sense in which laws in statute books are. They do not set out to prohibit any particular event, but allow everything, which is consistent with observable facts, to occur. This, it is suggested, means that every valid generalisation in physics can be stated so that the terms ' any ' and or ' either ' occur in its formulation. Progress towards unification in physics has been most rapid when physicists have acted on this assumption; and when specific assumptions have been made these have always had to be abandoned in favour of non-specific generalisations of the type defined above. The notion that valid generalisation in physics involves the use of the words ' either ' or ' any ' is formally stated as ' The Principle of Minimum Assumption '.

So far Professor Kapp has been concerned with establishing the philosophical bridgehead from which further advances can be made. Having stated ' The Principle of Minimum Assumption ', and described its status and analysed its philosophical basis, he then proceeds to apply it rigorously to the various possible hypotheses about the past and future duration in time of the material universe. There are three possible hypotheses about the duration of matter and energy in the past: that they have existed for an infinite time; that they have existed for a finite time; and that they have existed for any period of time. There are three corresponding hypotheses about the future duration of matter and energy : that they will exist for an infinite time; that they will exist for a finite time ; and that they will exist for any period of time. There are thus nine possible combinations of these hypotheses, each of which can be used as the basis for the development of a cosmological model. It will be noted, however, that only one of them contains the word ' any ' in both its constituent hypotheses. Application of ' The Principle of Minimum Assumption ', therefore, leads Professor Kapp to select this as the only valid generalisation. This philosophically derived hypotheses about the duration of matter and energy in the Universe is stated as follows :

> Any particle of matter or quantum of energy may have existed for any length of time . . . matter and energy are originating without cause, continuously, at random, and not as a result of anything in the existing state of affairs.
>
> Any particle of matter or quantum of energy may cease to exist at any time . . . matter and energy are disappearing without cause, continuously, and by extinction, at random, and not as a result of anything in the existing state of affairs.

This amounts to saying that matter and energy are being continuously created and destroyed, a view first advanced by Professor Kapp some thirty years ago. It is interesting to note that some twelve years ago Bondi and Gold independently arrived at the first part of this hypothesis, although for different reasons, which they published as the Theory of Continuous Creation.

The notion of continuous creation and destruction is called the Hypothesis of the Symmetrical Impermanence of Matter; and it obviously satisfies both the criteria embodied in Ockham's Razor and Professor Kapp's definition of a valid generalisation. Scientific hypotheses, however, must not only conform to philosophical criteria, they must also be predicative. In an observational, as opposed to an experimental, study the predicative capacity of a theory is investigated by using the theory to produce a model—in this case a cosmological model. The various properties of the model are then compared in detail with the observed activity in the external Universe; and one's attitude to the theory is then determined by the degree of correspondence exhibited in this comparison.

The cosmological model which is constructed on the basis of the Hypothesis of the Symmetrical Impermanance of Matter has many interesting features. The origin and evolution of the Galaxies is explained in terms familiar to those who have read the theory of continuous creation of Hoyle, Bondi, and Gold. Kapp shows that according to his theory one would expect that new clouds of matter would form in extragalactic space at finite intervals of time, and that their evolution into spiral nebulae should occur during a fairly short period of time during this evolution. The formation of the spiral arms should be accompanied by turbulence in a very large quantity of extremely tenuous gas. Although one would not expect this to lead to the emission of visible radiation it might cause radio-frequency emission. One might, therefore, expect regions in the neighbourhood of extragalactic nebulae to emit radiation which could be received by radio-telescope ; and such observation would be valuable confirmation of this particular cosmological model.

The most unexpected consequences of the Hypothesis of the Symmetrical Impermanence of Matter, however, are obviously those which follow from the hypothesis of the continuous destruction of matter. This leads to some unexpected conclusions. Kapp begins this section of the work by considering the various meanings of the word ' mass '. He distinguishes three meanings: inert mass; attracted (weight) mass; and attracting mass. He goes on to say that General Relativity Theory is based on the identity of inert mass and weight mass but that no satisfactory explanation of attracting mass has ever been given, beyond the statement that a particle carries associated with it an extensive gravitational potential gradient. Einstein pointed out that in regions of space curvature one can infer that a body free from restraint and possessing inert (and hence ' weight ') mass is accelerated. Now if the body is near an accumulation of inert mass it is observed to be accelerated; and therefore one assumes that space, in the vicinity of an accumulation of inert mass, is curved. This does not show that it is in the nature of inert mass to *cause* curvature of space but merely that it is in the nature of inert mass to *follow* curvature. General Relativity Theory goes no further than this; and hence in the past we have merely been able to say that every particle possessing inert mass has associated with it some curvature of space.

If, however, we assume that whenever a particle is created some space is also created, and that similarly when a particle becomes extinct, some space also becomes extinct we may make the following inference. During a particle's continued existence it has no attracting mass, but only inert and weight mass. When it becomes extinct, a local contraction of space occurs which manifests itself as a local curvature of space. This local contraction does not remain stationary, but travels outwards as a pulse ; this constitutes a gravitational field; and thus gravitation is quantised and has a finite velocity of propagation. Furthermore, since the total number of particles becoming extinct in any given period of time in any assemblage is a function only of the total number of particles present two further conclusions follow. The strength of the gravitational field associated with a given number of particles is proportional to that number, and hence bears a constant ratio to the inert and weight mass of the assemblage; and also one may talk about the half-life of matter in the same way in which one discusses the half-life of radio-active elements.

Professor Kapp arrives at a value of about 4×10^8 years for the half-life of matter. If this is so it means that the mass of the Earth and the other planets has been continuously decreasing since the time of their formation.

From this follow a number of remarkable astronomical, geological, and biological implications ; and one fancies that not all of these will be readily accepted by exponents of these particular disciplines.

One further consequence of the hypothesis that space is intensely curved in the neighbourhood of very dense aggregates of matter is that in the vicinity of atomic nuclei, gravitational forces are sufficient to account for the cohesion of nuclei. It is thus no longer necessary to postulate additional short-range nuclear forces.

The above inadequate resume is sufficient to show that Professor Kapp has produced a very remarkable book, of a type which is all too rarely written. It is essentially readable; it contains very little mathematics; and as has already been pointed out it is concerned in a very proper way with the philosophy of science. One is particularly impressed by the organisation of the material, the cogency of the arguments and the lucidity of the presentation. This book should be read by all those scientists who are concerned with the philosophical bases and status of their subjects and by philosophers who are interested in the development and meaning of scientific theories. Not everyone will agree with all of Professor Kapp's conclusions; but those who disagree will be stimulated by the ideas put forward and one suspects that the author hopes that this may happen. For in this way progress in generalisation in physics will occur; and on this road, 'Towards a Unified Cosmology' will be a very significant milestone.

H. D. TURNER

Modern Philosophy of Science: Selected Essays. By Hans Reichenbach. Translated and edited by Maria Reichenbach. Foreword by Rudolf Carnap.
Routledge & Kegan Paul, London; Humanities Press, New York, 1959. Pp. x + 214. 28s.

THIS fair sample of the almost two hundred writings of Reichenbach (1891-1953) has been edited by his wife, who is to be thanked for her careful work. It is useful both as an introduction to Reichenbach's thought and as a document for tracing the origins of most of his later, more elaborate, publications. In a single compact, well printed and bound volume, we can contemplate the birth of ideas which are more difficult to grasp when fully developed and couched in a highly technical and personal language.

The main subjects discussed in the eight essays contained in this volume are: relativity, causality, probability, and ethics. The first essay, written in 1921, is a somewhat aged discussion of the criticisms raised against relativity by fictionists, Machians, and neo-Kantians; it also expounds Schlick's and Reichenbach's own views, which were definitely favourable to Einstein's theories. One wonders whether at that time Reichenbach (who had just published his first book on relativity) clearly grasped the physical ideas he dealt with: in fact, the paper—which is cogent on the philosophical side—contains several mistakes. For example, the contention that in general relativity only coincidences are invariant (a basic condition for an operationalistic interpretation), whereas the metrical relations between the coincidences are relative—as if the line-element and the differential equations were not invariant, and as if they made reference to coincidences. Another mistake is the assertion that the Lorentz formulae express the causal relationships between the observations in different systems: only their empirical test requires a translation into an observational language, but even so co-ordinates do not acquire a causal efficacy. Also misleading is the assertion that the geocentric and the heliocentric descriptions of planetary motions are equally correct, a contention repeated by Reichenbach in later works; there is no such equivalence, among other reasons because the principle of equivalence is a differential law claiming no validity in extended regions of space-time. A further mistake is the statement that, for velocities faster than light, the kinetic energy becomes infinite: actually, it becomes complex. The philosophical discussions are, however, enlightening.

Reichenbach's probabilistic conception of causality, discussed in three of the present essays, rests on the remark that the statement of any (quantitative) law of nature, whether causal or not, should be supplemented by an hypothesis about the probable error—whence it is inferred that ' probability laws and causal laws are logical variations of one and the same type of regularity '. This view overlooks the peculiar logical status of statistical laws, which are general but not universal, and the semantical gap separating law statements from statements about law statements (e.g. ' Law L is exact to within 1 per cent '). Whereas law statements refer to things and processes, statements about laws and their test or application belong to a different level. Causation and chance are certainly not external to each other, and one should sympathise with attempts at disclosing their relations, but such attempts require a deeper logical (and ontological) analysis. The same is true of determinism, which Reichenbach equates with predictability, an identity which enables him to dispose easily with the former, since no completely exact quantitative predictions are possible. But determinism, an ontological theory, cannot be equated with predictability, a human capability, among other reasons because there are as many kinds of predictions as kinds of laws—in particular, it is possible to predict the evolution of frequency distributions.

Similar criticisms may be raised against Reichenbach's marriage of induction and probability: namely, that such a union is effected by a definitional stipulation. Induction ' is at the core of the theory of probability ', as Reichenbach maintains, if and only if an inductive definition of probability is chosen—for instance, as the limit of relative frequency. But what if the technical objections of the mathematicians are taken into account and if an implicit definition is adopted, in which the term 'frequency' does not occur—as is the case with Kolmogoroff's and Popper's theories of probability? Induction does occur, not however in the mathematical theory itself (which is formal) but in some of its applications—as, e.g., when we speak of (observed) stable frequencies in the long run. A further logical weakness is found in the very use of the term 'probability law' (for statistical law): it should be applied only to the axioms and theorems of the theory of probability, since 'probability' designates a mathematical, not an empirical concept. Again, the ambiguous term 'probability statement' should be used with care: one should distinguish between the frequency of collections of events, the probability of single events, and the probability of statements. The penalty for overlooking these distinctions is Reichenbach's proposal of the adoption of many-valued logic, on the ground that 'probability statements are not meaningful within a two-valued logic that requires every statement to be either true or false'.

The essays on ethics, which were written in 1951 and 1952 and are published posthumously, are probably the most interesting of all. They show us a rigorous and vigorous philosopher who has overcome some of the taboos of his youth to the point of dealing seriously with the freedom of the will. Freedom of action is characterised by certain probability implications (among them, 'The probability that in a situation A, a volition VB will lead to the action B, equals approximately one') supplemented with certain requirements (e.g. the causal relevance of the volition to the action). The probability conditionals are then interpreted as causal conditionals contrary to fact, and finally the following definition is reached: 'An action is free if it would have been influenced by a preceding volition'. The reviewer does not know of a more adequate logical analysis of the *potestas agendi*. The treatment of free will, on the other hand, does not seem to be as satisfactory. The definition of free will is just a specialisation of that of free action: 'A volition is free if it would have been influenced by a preceding volition'. The interpretation of freedom of the will as self-determination is dismissed, on the ground that the term 'internal cause' is vague—as if 'volition', a primitive in this study, were less vague. Finally, the cognitive explication of ethical discourse is discussed. While accepting that a set of valuations is involved in the taking of any given course, and that the knowledge of those valuations takes part in conscious action, Reichenbach is concerned with showing that the transition from the cognitive conclusion 'The action *A* will supply maximal satisfaction of the set of values *V*' to the action *A* is not a cognitive affair, since one might refuse to do A. He espouses the non-cognitive (Humean) theory of ethics, according to which imperatives are not derivable from declarative statements. While the core of this theory is true and important, it still remains a negative theory that does not satisfy our longing for a rational and empirical foundation for ethical 'utterances': that the Stoics failed in this enterprise should be regarded as a challenge rather than as a demonstration of the impossibility of giving ethics a scientific foundation.

The volume ends with a useful exhaustive Bibliography including Reichenbach's early articles on youth and students' problems, and which suggests that there is room for another volume containing some of his scattered essays on logic and logical analysis. For Reichenbach's most solid contributions, apart from *The Philosophy of Space and Time* and from his various *apologiae pro philosophia scientifica*, seem to be his logical works, which are not marred by a phenomenalist epistemology and an inductivist methodology.

MARIO BUNGE

The Way Things Are. By P. W. Bridgman.
Harvard University Press & Oxford University Press, 1959. Pp. x + 333, 45s.

LOOKING back on a double career as a Nobel Prize physicist and an amateur philosopher of great influence, Professor Bridgman is well entitled to air his views on Things in General.

Any man's personal wisdom is bound to be idiosyncratic (and Bridgman was never one to balk at idiosyncrasy), and so the value of the book to a reader will depend largely on his prior acquaintance, and sympathy, with Bridgman's views. To the unsympathetic it will appear as something of a mixed bag, containing very sharp insights together with reflections appearing naive at least. Arranged in one reviewer's rough order of increasing quality, there is the theory that a graduated income tax is the first step to Communism, the disbelief in the actual infinity of aggregates, the conviction that physics took a wrong turning with Planck's solution of the radiation problem, thought-experiments on relativity (a rotating searchlight whose beam transverses a distant nebula at more than the speed of light), a critique of the 'atomic' ideal in non-physical sciences, and a discussion of the meaning of 'event' in a physical system without particles.

Bridgman feels that there is an idea underlying all these various reflections: that we can't get away from ourselves, while physics generally proceeds on the assumption that we can. This may indeed be the case, and it would be valuable to see the idea applied as a tool of philosophical analysis, so that its strengths and limitations could be revealed. This is what Bridgman did with the 'operational' idea in his *Logic of Modern Physics*, and the positive residue of the insight has passed into the stock of common assumptions of modern philosophy of science. But in the present book, the basic insight is only a starting point for personal reflections on 'the way things are'—and so the appreciation and evaluation of the work is, unfortunately, a matter of taste.

J. R. Ravetz

Physics in my Generation. By Max Born.
Pergamon Press, London and New York, 1956. Pp. 232 + vii. 40s.

Twentieth-century physics rests on principles established in earlier centuries. But it has rejected three principles which as late as 1900 seemed unlikely ever to be displaced, namely determinism, absolute space and absolute time. There were preliminary indications of doubt, but in each case there was one penetrating and dramatic analysis which marked the beginning of the new mode of thinking. For determinism this was Max Born's paper 'On the Quantum Mechanics of Collision Processes'.[1] For absolute space and absolute time it was Einstein's paper 'On the Electrodynamics of Moving Bodies'[2] which, by fusing them into an absolute space-time, was decisive.

Indeed with the exception of Einstein and possibly Bohr no physicist in this century has contributed more than Max Born to the reassessment of the problem of explanation in physics. For the philosopher of science, Born's contribution to the twentieth-century revolution has been more radical than Einstein's, and (perhaps for just this reason) has taken longer to attract the attention of non-physicists. It has also not been quite so clear-cut. For this two reasons may be adduced. In the first place, Bohr had already presented thirteen years earlier a theory of atomic spectra in which determinism was provisionally suspended. Bohr's theory was logically incomplete in that while using Newton's mechanics to describe the motion of electrons inside atoms, together with a new principle to select only discrete orbits as possible in nature, it ascribed radiation to an unanalysed 'transition' process, in which determinism played no part. A logically self-contained theory emerged in 1925 and 1926 from the fusion of two apparently unrelated, but in fact equivalent approaches. The first, Heisenberg's abstract matrix mechanics, in which Born (whose assistant Heisenberg was) had played a prominent part, was at first difficult to interpret physically. The second was Schrödinger's wave mechanics, which its author hoped would establish atomic behaviour as an undulatory motion of a continuous medium. During this period, development in the application of 'quantum mechanics'

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[1] Max Born, *Z. f. Physik*, 1926, **37**, 863

[2] Einstein, *Annalen der Physik*, 1905, **17**, 891

(a name coined by Born) was so rapid, that the decisive overthrow of determinism in Born's paper, in which he showed that Schrödinger's wave mechanics could be used to explain collision processes, and that this required the wave-function to be interpreted as determining a probability, and not (as Schrödinger had believed) a continuous density, was not immediately differentiated from the wealth of other new and valuable results whose philosophical importance was less, nor recognised as the decisive step it really was. In the second place, with characteristic modesty, Born, while giving the clearest reasons why determinism probably had to be abandoned, and showing that in its non-deterministic form the theory was at least logically self-contained, left the possibility open that someone more penetrating than himself might yet formulate the theory in a deterministic way.

To those familiar with the intimate history of physics in the first half of this century much of this will emerge from between the lines of *Physics in my Generation* in which Professor Born collects together his popular writings over a period of thirty years. But Professor Born is not good at blowing his own trumpet, and is uneasy when he is dealing with just those branches of physics where his own contributions have been the greatest. He is at his best when analysing the thought of his friend Einstein, for here no feeling of modesty is present to restrain him; and his own deep insight into physics illuminates the workings of the creative process which he understands so well.

The articles gathered together here are of several kinds: prefaces and postscripts to books, popular lectures, semi-popular review articles. Not all are concerned with the philosophy of science. Some, towards the end (the order is strictly chronological) are concerned with the moral issues raised by nuclear weapons, and by the pursuit of objective truth for its own sake. The most substantial, however, are concerned with the rôle of theory in physical science.

Many good physicists are uneasy when trying to analyse their activities from a philosophical standpoint, and in spite of his success at it, I suspect that Born is often among their number. Preoccupation with the philosophy of a science requires the cultivation of the significant generalisation, but every generalisation involves definition and delimitation, and the latter can be the enemy of scientific progress. Occasionally one glimpses reminders in these essays that all is not as in the writings on ' scientific method '. For example (p. 30):

> At first, however, they (the formulations of quantum mechanics) were only formalisms, and it was a matter of discovering their meaning *a posteriori*. It is in fact, very common in physical investigations to find it easier to derive a formal relation from extensive observational material than to understand its real significance. The reason for this lies deep in the nature of physical experience:

> the world of physical objects lies outside the realm of the senses and of observation, which only border on it; and it is difficult to illuminate the interior of an extensive region from its boundaries.

Occasionally, too, Born's generalisations, condensed into a pithy sentence, raise disagreement or doubt, but challenge one to look at some of the deepest problems. For instance (p. 125):

> He (Bohr) stressed, from the very beginning, the new features of the scheme, namely the indeterministic character of the transitions, the appearance of chance in the elementary processes. This means the end of the sharp separation of the object observed and the subject observing. For chance can be understood only in regard to expectation of a subject.

The last sentence gives me a jolt. I do not know whether I agree with it or not, but I am forcibly reminded that behind the quantum theoretical requirements on the concept of probability, to be defined even for a single elementary (and possibly quite unrepeatable) event, there lies a problem of logic whose solution I do not know. Or again (p. 89):

> I cannot see how the Bose-Einstein counting of equally probable cases can be justified without the conceptions of quantum mechanics.

Here I personally disagree with the implication, but once again I am jolted into looking critically at an important principle.

I was impressed by the concept of 'style' in science, which Born takes over from Pauli in the Guthrie Lecture (pp. 123-138), and again in his rejoinder to Schrödinger's 'Are There Quantum Jumps?' (p. 150). Each age of science has its characteristic style, and the style of our century is quite distinctive. And if one wants to learn to appreciate this style, short of contributing oneself to its creation one can hardly do better than to read *Physics in my Generation.*

M. H. L. PRYCE

A History of Science. Vol. 2: Hellenistic Science and Culture in the Last Three Centuries B.C. By George Sarton.
Harvard University Press; London: Oxford University Press, 1959.
Pp. xxvi + 554. 63s.

THE late George Sarton devoted his life to the study and dissemination of the history of science. *Isis*, the review he founded in 1913, is now the leading journal in this field; the critical bibliographies which he instituted as an annual feature of *Isis* have been of inestimable value to scholars in many areas; his *Science and the New Humanism* is still the most eloquent plea for the unification of Snow's two cultures; the monumental five volume *Introduction to the History of Science* is indispensable to the serious student of ancient and medieval science.

In his later years, Professor Sarton saw the necessity of completing his scholarly career by weaving together the many strands of his researches into a single synthesis. His hope was to produce a history of science in eight volumes in which the evolution of scientific ideas from antiquity to the present day would be traced in detail. When he died in 1956, however, he had just completed the present volume, and it must constantly be kept in mind that he was unable to see this work through the press. Some of the short-comings noticed below might have been eliminated had Sarton lived.

The volume is divided into two parts: the first deals with the third century B.C., the ' Alexandrian Renaissance '; the second with the last two centuries before Christ, when the pall of Rome was slowly being cast over the Mediterranean world. In each part, Sarton sets the scene with a description and analysis of the social and cultural milieu in which the scientific achievements took place. Chapters are then devoted either to the work of men like Euclid and Archimedes or to general fields such as geography, anatomy, natural history, and medicine. To complete the map of the intellectual world, there are chapters on philosophy and religion, history, philology, literature and the arts.

The work as a whole is very uneven and suffers considerably from the fact that Sarton's aim far exceeds his reach. That he was correct in insisting that the history of science must be written within the larger framework of cultural history cannot be denied; but to do this, the author must be able both to understand and explain the scientific aspects of a given period, and to see the past in historical perspective. There can be no doubt about Sarton's scientific competence but his view of history is almost painfully naïve. The *dramatis personae* are divided into ' good guys ' and ' bad guys ', and customs into ' good practises ' (writing plain, rather than effusive, dedications; supporting science) and ' bad practises ' (slavery and oriental superstitions). From this clash of black and white, one supposes, comes the dynamic of history.

In the non-scientific chapters, Sarton carries his documentation to an extreme. Must we be informed by a footnote that Hannibal ' was the greatest Carthaginian general ' (p. 13, footnote 28), or that the correct pronunciation of Japan's largest city is ' Tōkyō ' (p. 9, footnote 19)? Is it essential that we be told that the popularity of the Winged Victory and the Venus de Milo is partially due to their being in the Louvre, and then exhorted to ' remember that a great many works of art have been shown in the Louvre for centuries without becoming popular ' (p. 510, footnote 27)? A ' footnote frequency ' of four per page, consisting in large part of such trivia, surely can be described only as pedantry.

Finally, these chapters suffer from two closely allied faults which arise from Sarton's laudable desire to present a complete picture of the Hellenistic intellectual world. On the one hand, he is led into relatively lengthy digressions which serve no useful purpose and will confuse or bore many readers whose main interest happens to be the history of ancient science. Thus the discussion of the Milindapanha, an Indian dialogue between King Milinda (a Greek) and a Buddhist priest, displays Sarton's admittedly vast learning but detracts from his narrative. In other instances, he falls back on a general catalogue of names, dates and works which could easily be found in a classical dictionary. In a book written expressly for ' men of science who are anxious to know the origins of their knowledge ' (p. xi), what are we to make of the following (complete) entry?

> Philodemos of Gadara was an Epicurean poet, a contemporary of Cicero. His poems (some thirty) were eventually included in the *Crown*, not in the first edition, but in the second, prepared by Philippos of Thessalonice (*c.* A.D. 40) (p. 460).

Since fourteen of the twenty-nine chapters deal with non-scientific matters, these criticisms cannot simply be dismissed as referring to areas peripheral to the central theme of the work.

In his discussion of the state and development of science in the Hellenistic period, Sarton is on much firmer ground. The union of his unsurpassed bibliographical talents and his profound knowledge of ancient science provides unique chapters in which the reader not only finds the body of ancient science revealed, but also has the road of transmission from antiquity through Byzantium, Islam, and medieval Spain and Sicily mapped for him.

Curiously enough, these chapters are open to the criticism that they fail to deal with ancient science in sufficient detail. Does not Archimedes' proof of the law of the lever, which figured so prominently in the development of statics and, with Mach, in the history of the philosophy of science, deserve reproduction here? Should not the *Conics* of Apollonios be given greater space? Raised on analytical geometry and the calculus, how many of us, without special training, could handle the problems treated in that work? How much real understanding of higher Greek mathematics can we have if we are told only that the ellipse, the hyperbola, and the parabola can be dealt with by geometric methods? How tantalising to be informed that Book V of the *Conics*, ' Maxima and Minima ', was Apollonios' masterpiece and never be given even a hint as to how Maxima and Minima, without the differential calculus, could be determined!

It is not the province of a reviewer to suggest the kind of book an author should have written. In this case, however, I cannot help but wish that Sarton had cast his net less widely. Had he dealt with Hellenistic science in more detail and allowed his vast erudition to embellish rather than overpower this tale, there can be little doubt that this volume would have been the best memorial to a life spent in the service of scholarship.

L. PEARCE WILLIAMS

The Science of Mechanics in the Middle Ages, 1200–1400. By Marshall Clagett. University of Wisconsin Press, 1959. Pp. xxix + 711. $8.00.

'HISTORY', wrote Lord Bolingbroke, 'is philosophy teaching by example'. Like most epigrams, this one has been extensively quoted and only rarely applied. Yet, in certain areas, this approach can be utilised to great advantage both to the historian and to his audience. Consider, for a moment, the plight of the historian of medieval science. His raw material consists of texts of considerable philosophical difficulty, in many cases containing complex and ingenious mathematical and physical reasonings and written in languages which few people today command well enough to follow the substance of the argument. Moreover, he is flanked by the holders of two opposing prejudices. There are still those who insist that medieval science was mere philosophical vapourings concerned either with the number of angels who could dance on pin heads or, as 'applied philosophy', with the magical essences of gems, fabulous beasts, and so on. At the other extreme are the followers of Pierre Duhem who view modern science as a rather extended footnote to the medieval achievement. How can the historian of medieval science present his subject in such a way that he can be analytical and, at the same time, avoid the charge of prejudice or textual misreadings from his critics?

Bolingbroke provides one solution and it is this method which Professor Marshall Clagett has adopted in this latest volume of the University of Wisconsin Publications in Medieval Science. The whole area of medieval mechanics is examined in what, to my mind, is an exemplary fashion. There are four parts: Medieval Statics, Medieval Kinematics, Medieval Dynamics, and The Fate and Scope of Medieval Mechanics. With the exception of Part IV, each section consists of a number of analytical chapters in which Professor Clagett discusses with great skill the development of various aspects of medieval mechanics, and then illustrates these developments with extensive selections from medieval documents. In Part IV, Professor Clagett sums up the preceding 625 pages in two excellent chapters. The first, 'The Reception and Spread of the English and French Physics, 1350–1600', is an admirably balanced account of the fate of the ideas developed by the Schoolmen at Merton College and Paris during the thirteenth and fourteenth centuries. The final chapter, 'Medieval Mechanics in Retrospect' considers the contribution of medieval man to the science of mechanics. In these two chapters Professor Clagett covers much the same ground as Duhem in his three volume *Etudes sur Léonard de Vinci* (Paris, 1906–13) and Anneliese Maier in her five volumes on medieval mechanics. Duhem's somewhat over-enthusiastic claims for medieval science are corrected and Dr Maier's views nicely summarised. For the student who desires a precise evaluation of the debt of classical mechanics to the Middle Ages and who does not care to work his way through Dr Maier's excellent volumes, these two chapters of a little over fifty pages may be recommended without reservation.

To a large extent, the faults of this volume are the direct result of its virtues. Being partly a volume of sources and partly a history of medieval mechanics, it ends up being wholly neither one nor the other. It is certainly more than a source-book of medieval mechanics; but, because of the inclusion of sources, Professor Clagett necessarily must spend part of his time discussing their origins and these discussions sometimes seem to be more detailed than one would think necessary. The hybrid nature of the work is not a very serious fault, however, and Professor Clagett's forthcoming history of Greek science in the Middle Ages should provide the continuity sometimes missing here. This volume is clearly the foundation on which Professor Clagett intends to build and, as such, it could hardly be better.

This is a handsome book, well-printed and well bound. There is an excellent bibliography and index. The price ($8.00—£2 17s. 2d.) is well worth paying for a work which will reward those who have an interest in the history of science and the history of medieval thought.

L. PEARCE WILLIAMS

Gödel's Proof. By Ernest Nagel and James R. Newman.
New York University Press, 1958. Pp. 118. $1.75

IN one of the most significant mathematical papers of the past forty years Kurt Gödel published in 1931 a proof that the consistency of arithmetic, and hence of the whole structure of mathematics which has been built on it, is incapable of proof by methods which are susceptible of formalisation within arithmetic itself. In the same paper Gödel also showed that no formalisation of arithmetic can contain the whole of arithmetic in the sense that within *every* formalisation there are propositions $P(n)$ such that each of $P(0)$, $P(1)$, $P(2)$, . . . is provable but the universal proposition $(\forall n)\, P(n)$ is not provable. As was subsequently (and independently) shown by Th. Skolem this means that every codification of arithmetic admits a non-standard model, i.e. an interpretation in which a class of objects other than the natural numbers plays the number rôle.

The present account by Nagel and Newman concentrates upon explaining the background notions like consistency and formal proof; there is also a sketch of a method of numbering formulae which is used in the Gödel proof and a hint of one of the central ideas of the proof. But all non-technical expositions of Gödel's work cannot take the reader to the heart of the matter, which concerns the *nature* of the arithmetical functions which represent the meta-mathematical properties *via* the Gödel numbering.

The importance of Gödel's result for the philosophy of science is in its clarification of the notion of a formal system, and the limitations of the axiomatic method. Gödel's discoveries, however, are not in conflict with views about the nature of mathematical entities radically different from Gödel's own views. Gödel sees mathematics as something which exists in advance of its discovery, so that a formalisation of arithmetic is a shadow to be compared with the real thing—arithmetic itself. But those who hold that, by and large, mathematicians are inventors, not discoverers, do not feel driven either to naïve realism or to Platonic idealism to account for the fact that a game with strict rules is not identical with one less sharply delimited.

R. L. Goodstein

Induction and Hypothesis. By S. F. Barker.
Cornell University Press; London: Oxford University Press, 1957.
Pp. xvi + 203. 22s.

Mr Barker has written one of the most carefully argued and conclusive cases that I have seen against all attempts to justify non-demonstrative scientific inference in terms of any type of induction, confirmation, or formal hypothetico-deductive method. He will not, however, like this as a conclusion of his argument, for he ends by proposing yet another modification of the formalist point of view. But before coming to that, it must be said at once that he has provided an excellent summary, in non-technical language, of the pros and cons of most of the main logical theories of non-demonstrative inference. From this point of view the book would make a useful text book in a field where good introductory works are rare.

Starting with the very reasonable postulate that since we do in fact make choices between rival empirical hypotheses on the basis of evidence, there must be in some sense rules of choice between them, Barker first clears the ground by discussing the kind of hypothesis and the kind of evidence he intends to be concerned with. With regard to laws or theories, it is not to be demanded that they carry any necessity; and with regard to observation statements, no decision is to be made as to what kind of terms they should contain, that is, whether they should be sense-data or physical-object statements, or whether their truth or falsity should be assumed to be certainly knowable. It is demanded only that they be singular existential statements such as sometimes admit of verification by observation. In other words, the starting-point is consistent with most forms of empiricism.

The author then considers eliminative induction (Keynes), and enumerative induction, including Carnap's system, and rejects these, both for internal reasons and for the general reason that no such theory of induction can account for our preference for simple quantitative laws, nor for the confirmation of hypotheses concerning unobserved entities, or containing theoretical terms (these latter, however, he later finds reasons for excluding from the class of significant scientific terms). There follow criticisms of logical constructionism and of formalism as accounts of these kinds of hypotheses, and the author then passes to the method of hypothesis not based on induction, and considers the difficulties of Popper's view that the acceptability of hypotheses should depend on their falsifiability. Finally, Kemeny's measure of the number of ways in which statements of a hypothesis in a given language could come out true in an *n*-member universe is used to suggest a method of comparing the simplicity of hypotheses, and a criterion of choice between two hypotheses is derived. A notable omission is any discussion of Braithwaite's account of choice between statistical hypotheses in terms of policies designed to maximise gains or minimise losses, or the analogue of the theory of games.

The author's claim for his own suggested method is a modest one, namely that it allows some simple kinds of hypotheses to gain confirmation on the basis of favourable evidence, and that conflicting hypotheses may be compared in relation to a given body of evidence. But one of his conclusions is that his method implies that no unobservable predicates ought to be introduced into theories, since predicates ' which have not been observed to have any instances ' cannot increase the simplicity of a hypothesis in his sense. As he quotes as examples of such predicates ' atoms ', ' electrons ', and ' genes ', it seems, first, that this conclusion constitutes a serious objection to his method, and second, that he has departed from his initial intention not to stipulate what kinds of terms ought to be allowed to enter observation statements. It is really not very helpful to be told, after thirty years of operational analysis, that

> any extralogical term legitimately being used in science . . . in principle ought somehow to admit of being defined by means of observational predicates which do occur in the evidence. Such a definition may be quite complex and difficult to supply . . . a formidable task . . . (p. 198).

The author claims that his proposal is not reductionism, because although he does not allow for the occurrence in theories of unobserved *predicates*, he does allow for unobserved *entities* having observable predicates, for example, an unobserved planet detected by its observable gravitational effects. This is not, however, much of a concession from the point of view of theoretical science, for even ordinary discourse cannot do without it, and the peculiarly scientific problem (at least as it has appeared to formalist philosophers) is not that of unobserved entities but of unobserved predicates.

If, in spite of its rigour and clarity, one puts this book down with a feeling of disappointment, it is surely because one is reminded of the fate of the astronomy of epicycles. Formalist philosophies of science which cannot, for all their logical ingenuity, give a more realistic account of scientific reasoning than this, will surely pass into the same limbo, assisted thither, I am afraid, by some of the destructive arguments of this book.

M. B. HESSE

Science and the Idea of God. By C. A. Coulson.
University Press, Cambridge, 1958. Pp. vi + 51. 4s. 6d.

The Pious Scientist. By James K. Feibleman.
Bookman, New York, 1958. Pp. 111. $3.00

THE first of these slim volumes represents the eleventh of the Eddington Memorial Lectures.

Like several others in this distinguished series it takes Eddington's ideas as a starting point rather than a theme, and the ground covered will in some measure be familiar to readers of Professor Coulson's other religious works. What is new in it, however, is not without interest for philosophers of science. Coulson is convinced (with reason, it would seem) that arguments from the *substance* of science cannot validly support belief in God. But where Eddington could speak of the 'security of our relationship with God' as sufficient to 'turn aside . . . the most convincing disproof' Coulson is not satisfied. 'In order to find God' a scientist must look, not indeed to the substance, but 'to the manner of (his) inquiry, the presuppositions which inform it, the corporate nature which it shares and his own human reactions and response to what he finds within it'.

There are acknowledged similarities in thought here to Polanyi's great thesis, particularly as developed later in his *Personal Knowledge,* and this whole line of thought deserves more attention than it has generally yet received. What may be questioned, however—and not only by unbelievers—is the extent to which the 'faith' and other attitudinal characteristics of a scientist can bear the specifically religious weight that Coulson would hang on them. Given Christian commitment, one's scientific presuppositions and attitudes do indeed find religious sanction; but is there enough (or even anything) in scientific experience as such to impel to Christian commitment? Not, indeed, 'unerringly', says Coulson (p. 49); but rather in the way that the clues in a detective story point to the criminal—becoming compelling only when the plot is uncovered. The analogy is appealing; but alas, even a full sketch of the theistic dénouement apparently fails in practice to convince some scientists that the 'clues' in their discipline have been fairly planted; and since Christ himself seems to have indicated a more operationally satisfying line of enquiry (*John,* 7: 17) they may well be right.

As an essay in the integration of faith and thought, however, this is a notable and thought-provoking booklet, rich in apt quotation, and informed throughout with that robust yet poetically sensitive commonsense for which the author is beloved by those who know him.

Professor Feibleman is Chairman of the Department of Philosophy at Tulane University. His theme: ' An uncommitted inquiry into the nature of the unaffiliated truth, that is what is wanted ' (p. 8). A sample from his ' New Creed ': ' Avoid harm to others; Be altruistic to stones; Stay on the positive side '. The relevance of this book to the philosophy of science is mainly cautionary.

D. M. MacKay

Turning Points in Physics. A Series of Lectures given at Oxford University in Trinity Term, 1958. R. J. Blin-Stoyle, D. ter Haar, K. Mendelssohn, G. Temple, F. Waismann, and D. H. Wilkinson, with an introduction by A. C. Crombie.
North-Holland Publishing Company, Amsterdam, 1959. Pp. 190. 21s.

The essays making up this book are taken from the lectures given to Oxford undergraduates, by a group of five physicists and one philosopher. Dr Crombie's only explicit contribution is a brief introduction, but the essays speak for his skill in selecting and ' briefing ' his speakers. The bulk of the book is devoted to explaining, to non-specialists, the conceptual and philosophical developments associated with the revolution in physics which took place in the first half of this century.

I shall now examine the book on an unfair basis, and see what it has to offer to readers of this *Journal*, who may be assumed to have some acquaintance with the material discussed. Historical essays such as those written in this collection by the physicists are more valuable as documents of the reflections of first-rate scientists on the history and philosophy of their subject, than as sources for the history itself. Still, they are useful summaries of developments of a period to which the professional historians have not yet turned their attention. Dr Mendelssohn's essay serves as a forceful reminder of how fortunate is contemporary physics; in a conceptual revolution where common ideas of space, time, and individuality are thrown overboard, a confused and paradoxical notion of probability somehow serves to carry an enormous burden of physical theory built upon it. Professor Temple's essay is the most stimulating and charming, probably because he concentrates on a conceptual, rather than a historical, analysis of relativity.

Dr Waismann's long essay throws light on various parts of the long and complicated history of such concepts as ' causality ' and ' determinism '.

One becomes so well convinced of the complexity of these notions, that one cannot feel comfortable with the statement of their destruction by a single formula (Heisenberg's) and its interpretations. Although Dr Waismann's examples of wave and quantum effects are most beautifully done, the reviewer has reservations about the accuracy and adequacy of some of the examples used in the discussion of classical physics. A given (finite) sequence of discrete events (number-pairs) does not need Fourier's Series for a 'law' (p. 99)—a polynomial will do nicely, and will be no more or less trivial from the theoretical point of view. The dramatic example of the ball on the round billiards table (with periodic path if the angle of impact is, in radians, rational, and with non-periodic path otherwise) is not well suited for the moral that predictability depends on complete accuracy of initial information. To determine whether a quantity is rational or irrational is not an operation of physics; moreover, to determine whether any given path is periodic is impossible in a bounded time-interval. The example shows neatly the great difference between mathematics and physics: the mathematical realisation of the initial mental image turns out to have quite interesting properties which have no physical meaning; the physical realisation might also have interesting properties (in this case it hasn't), but the mathematical formulation of those particular properties would quite possibly be of trivial mathematical interest at best.

So much of these first five essays is dovoted to showing students that the physical world is a queer place, that the history inevitably takes on the tinge of showing how wrong was all physics before 1905. And the von Neumann proof makes our present conceptions as near to synthetic *a priori* as is respectable nowadays. (The historian remembers that Zeno's two-pronged proof of the impossibility of a consistent description of space and time has not yet been refuted to the satisfaction of all interested parties.) This hardening of the metaphysical arteries is characteristic of completed revolutions, as Waismann himself points out with reference to Newtonian mechanics (p. 98). The other sign that the revolution is completed is the content of Professor Wilkinson's exciting article on elementary particle physics. Yesterday's paradoxes are today's truisms (don't expect to extrapolate successfully from the macroscopic to the microscopic world); yesterday's theories give no guidance for making sense of the mass of current experimental evidence. Now one must classify, and use heuristic principles (symmetries and conservations) for making shrewd guesses. As this sort of work continues, it may be that the relevant history will cease to be the reflections of Newton and Maxwell on the foundations of mechanics, but rather the work on airs, inflammable, fixed, phlogisticated, and respirable, of Scheele, Priestley, and Lavoisier.

J. R. Ravetz

REVIEWS

Revelation through Reason. By Errol E. Harris.
Allen and Unwin, London, 1959. Pp. 123. 15s.

THE surprising thesis of this essay is that sciences and religion are identical. Both are seeking knowledge of the final truth about the universe, and truth is all-inclusive and ultimately one. 'Religion . . . is the name for one's total conscious attitude toward life, as it is formed and enlightened by rational awareness and knowledge. Science in the narrower sense is an integral and indispensable factor in this, and in the wider sense which includes philosophy it is identical with it.'

According to Professor Harris, the world view of modern science, in all branches, is evolutionary. This is the picture of an ascending scale of forms, reaching completion in a reality than which nothing greater can be conceived. This leads to a reformulation of the theistic Proofs. The following form of the ontological argument is defended: nothing that is developing can be understood without references to its final state, and the final state of everything is God. Therefore God is the presupposition of all rational discourse. Therefore all putative disproofs of God's existence are self-contradictory.

Two comments: (*a*) The thesis of the identity of science-cum-philosophy and religion rests on a definition apparently picked for its blur-value. (*b*) The premiss for an evolutionary cosmology is that since the middle of the nineteenth century the idea of evolution has dominated scientific thought in all fields and guided almost every scientific investigation. This does not seem a strong basis for so far-reaching a generalisation.

R. J. SPILSBURY

ANNOUNCEMENT

BRITISH SOCIETY FOR THE PHILOSOPHY OF SCIENCE

Fifth Annual Conference : 23rd–25th September 1960

A conference will be held at Wills Hall, Bristol, from Friday evening to Sunday afternoon, 23rd to 25th September. The Conference fee will be £3 15s. 6d. for full residence and gratuities. This includes 10s. registration fee.

The programme will be as follows :

Friday evening :	'The Relation between Pure and Applied Mathematics', Professor S. Körner and Professor R. L. Goodstein.
Saturday morning :	'Universality and Necessity', Professor W. C. Kneale and Dr I. Lakatos.
Saturday evening :	'Learning to Perceive', Mrs M. L. J. Abercrombie and Dr M. Pirenne.
Sunday morning :	'The Relation between Methods of Investigation and the Characteristics of Physical Laws', Dr D. Bohm and Dr P. W. Higgs.

Those wishing to attend are asked to write to the Hon. Secretary, Dr M. B. Hesse, Whipple Museum, Free School Lane, Cambridge.

ABSTRACTS

Dialectica, 1958, **12**, Nos. 3/4

R. Péter, ' Graphschemata und rekursive Funktionen '

Kaluznin, in describing graph-schemata which could be used as definitions of arithmetical functions, suggested that if the functions were classified according to the complexity of the corresponding graph-schemata, it might be possible to give the intermediate levels for special recursive functions and general recursive functions. This paper shows that this is not possible.

A. Robinson, ' Relative Model-completeness and the Elimination of Quantifiers '

Most of the early proofs of the decidability or completeness of certain mathematical theories were based on the method of eliminations of quantifiers. Various more recent results on completeness were obtained independently of such procedures. However, it is shown in the present paper that, conversely, the completeness of a mathematical theory will in certain circumstances entail the existence of an elimination method. The proof involves the application of the extended first ϵ-theorem of Hilbert-Bernays.

H. A. Schmidt, ' Über einige neuere Untersuchungen zur Modalitätenlogik '

In the present report on some papers of the author and on a sequel to them by G. Emde, Marburg, a series of results concerning the combination of the basic modalities ' possibility ' and ' necessity ' are treated. Starting from some very general framing-codifications, the list of the finitely many implicative modal logics with idempotent ' possibility ' which is obtainable through basis reduction is discussed. Besides basis reduction, in the case of some important subclasses of the not-necessarily-idempotent implicative modal logics, attention is given to the special decision problems.

K. Schütte, ' Aussagenlogische Grundeigenschaften formaler Systeme '

In a formal system without types which contains no contradictions and is able to represent all notions of classical mathematics, all the laws of classical propositional calculus cannot be valid. From this fact ensues the problem generally to explore the properties of formal systems from the point of view of the propositional calculus. Under certain assumptions concerning the propositional expressions the ' propositional completeness ' and the ' propositional consistency ' of a formal system is characterised through the *tertium non datur*, respectively through the inference rule of the *ex falso quodlibet*. There are decision procedures for the syntactical inference rules which in every formal system are valid under the given assumption and for the syntactical inference rules of every formal system which is complete or consistent with respect to the propositional calculus.

ABSTRACTS

E. Specker, ' Dualität '

The axiom system of plane projective geometry is dual in the sense that it is transformed into itself by exchange of the notions ' point ' and ' line '. It follows that for every theorem the dual sentence is also a theorem. However, from the duality of the axiom system one cannot conclude that in a model the truth of a sentence implies that of the dual sentence; even less can one conclude that each model admits a 1-1 transformation interchanging points and lines and preserving the incidence relation. For projective geometry, models of this kind are well known. For the simple theory of types (where duality is replaced by ambiguity of types) it is shown that the existence of such models is equivalent to the consistency of 'New Foundations'.

Additional remark. The following theorem answers both of the questions proposed in the paper: If it is complete, then a theory with an automorphism has a model with a corresponding automorphism. NF is therefore consistent if simple theory of type with the additional axioms S S^* (in the notation of the paper) is consistent.

W. Ackermann, ' Uber die Beziehung zwischen strikter und strenger Implikation '

In this paper the author enters into the particulars of the relations of the concept of ' strict ' implication introduced by C. I. Lewis with the concept of ' strenge ' implication introduced by himself. He points out that within the system of ' strenge ' implication a further concept of implication may be defined which has all the qualities of strict implication. The definition is the following: A implies B in the sense if the conjunction of A and non-B is impossible, which is not the same as the ' strenge ' implication between A and B. The system of strict implication is taken in the form as formerly given by Arnold Schmidt.

E. W. Beth, ' " Cogito ergo sum "—raisonnement ou intuition '

According to Descartes, the demonstrative power of an argument may result either from the application of the universal rules of logic or from a particular intuition. This doctrine permits us to avoid the reproach of circularity which is often raised as an objection to certain argumentations in epistemology. On the other hand, it implies the rejection of the method of the counter-example. The acceptance of certain argumentations based on a particular intuition never creates a permanent situation; it rather constitutes a stage in the genesis of what Bernays has characterised as an *évidence acquise.*

R. Carnap, ' Beobachtungssprache und theoretische Sprache '

Among the non-logical constants of the language of science two kinds are distinguished, the observation terms (e.g. ' blue ') and the theoretical terms (e.g. ' electric field '). The latter terms are introduced, not by definitions, but by postulates of two kinds, theoretical postulates, e.g. basic laws of physics, and correspondence postulates which connect the theoretical terms with observation terms. As Hilbert has explained, both mathematics and theoretical physics can in this way be constructed in the form of uninterrupted calculi. It is here briefly indicated that by this method of construction also the mathematical terms have meanings (in a wider sense) assigned to them. The theoretical terms obtain at least an incomplete interpretation by means of the correspondence postulates. It is shown how the distinction between analytic and synthetic sentences can be defined also for the theoretical language.

ABSTRACTS

H. Wang, 'Eighty Years of Foundational Studies'

A survey is made of work since 1879 on foundational problems viewed as an analysis, by reduction and formalisation, of the concepts proof, feasible, number, set, and constructivity. It is suggested that there are five domains of concepts and methods, viz. anthropologism, finitism, intuitionism, predicativism, and platonism. It is also suggested that the central problem is to characterise these domains by formalisation and to determine their interrelations by different forms of reduction. Finally, the range of logic in the narrower sense is discussed, and applications of mathematical logic are briefly outlined.

H. B. Curry, 'Calculuses and Formal Systems'

Lorenzen, in his book *Einführung in die operative Logik und Mathematik* has given a relatively precise form of syntactical system which he calls a calculus. The present paper deals with the relationship of Lorenzen's notion of calculus with the notion of formal system. It is shown that the 'obs' of a formal system can be represented as the theses of a calculus of a certain type just when the calculus has a property called the tectonic property, and conditions are given under which one form of system can be transformed into the other.

K. Gödel, 'Über eine bisher noch nicht benützte Erweiterung des finiten Standpunktes'

P. Bernays has pointed out that, in order to prove the consistency of classical number theory, it is necessary to extend Hilbert's finitary standpoint by admitting certain abstract concepts in addition to the combinatorial concepts referring to symbols. The abstract concepts that so far have been used for this purpose are those of the constructive theory of ordinals and those of intuitionistic logic. It is shown that the concept of a computable function of finite simple type over the integers can be used instead, where no other procedures of constructing such functions are necessary except simple recursion by an integral variable and substitution of functions in each other (starting with trivial functions).

R. L. Goodstein, 'On the Mature Mathematical Systems'

The crux of the dispute between formalism and intuitionism, it is held, is not whether certain entities exist or not, but how the term function shall be used in mathematics. The identification of effective definition with general recursion fails because an undefined function lies concealed beneath the requirement of a finite number of substitutions, and a fresh characterisation of effective definition is sought in terms of a hierarchy of ordinal recursions.

A correspondence exists between primitive recursive properties and direct proofs, of irrationality and transcendence for instance, and between general recursive properties and indirect proofs.

Mathematics is a concept creating activity and the distinction between a formal mathematics devoid of meaning, at one level, and a meaningful metamathematics at the next is considered to untenable.

H. Hermes, 'Zum Einfachheitsprinzip in der Wahrscheinlichkeitsrechnung'

Shimony, Lehman, and Kemeny recently developed a foundation of the theory of confirmation by reduction to the concept of rational betting. This procedure yields essentially only the axioms first stated by Kolmogoroff. It is well known that these are not sufficient for application. Thus it is necessary to search for a new principle if one wants to motivate new axioms. This can be done by a principle of simplicity, which expresses that the probability of a hypothesis increases with its degree of simplicity. A critical survey is given about several attempts which have been tried in Munster especially by Kiesow and W. Oberschelp with the aim to make the notion of simplicity precise. The simplicity is reduced to syntactical properties of proposition.

A. Heyting, 'Blick von der intuitionistischen Warte'

The paper contains remarks on intuitionism and its relations with other domains of foundational research. Inside the intuitionistic mathematics, in connection with Griss's criticism against the use of negation, different degrees of evidence are distinguished, depending upon the way in which conditioned constructions are admitted. Some difficulties in the theory of finite species are discussed. Concerning the foundational research in general it is observed that it has separated intuitive, formal and platonistic constituents in classical mathematics. Some remarks are made on Church's thesis in the theory of recursive functions.

G. Kreisel, 'Hilbert's Programme'

Hilbert's plan for understanding the concept of infinity required the elimination of non-finitist machinery from proofs of finitist assertions. The failure of the original plan leads to a hierarchy of progressively less elementary, but still constructive methods instead of finitist ones (modified Hilbert programme). A mathematical proof of this failure requires a definition of 'finitist'. The paper sketches the three principal methods for the syntactic analysis of non-constructive mathematics, the resulting consistency proofs and constructive interpretations, modelled on Herbrand's theorem, and their mathematical and logical consequences. A characterisation of finitist proofs is sketched. A remark on the completeness of the predicate calculus concludes the paper. Throughout open problems and alternative approaches are emphasised.

ANNOUNCEMENT

Xth International Congress of the History of Science

The Xth International Congress of the History of Science will be held in the United States of America from 25th August to 2nd September 1962. Opening sessions of the Congress will be held at Cornell University, Ithaca, New York, and the concluding sessions will be held at the American Philosophical Society, Philadelphia, Pennsylvania. The President of the Congress is Professor Henry Guerlac of Cornell University. The Secretary of the Congress is Professor C. Doris Hellman.

All inquiries should be addressed to The Secretary, Xth International Congress of the History of Science, Cornell University, Ithaca, New York, U.S.A. Those wishing to receive bulletins concerning the congress are requested to communicate with the Secretary.

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SPACE AND SENSE-DATA*

W. RUSSELL BRAIN

I HAVE argued in the past that our knowledge of neurophysiology necessitates the adoption of some form of the sense-datum theory. The following are the chief reasons for this. I experience a sensation only when certain changes occur in the cells which compose my nervous system. When, by means of such sensations, I perceive an object outside my body, the sensations are the end-result of a long chain of physical causes starting at the object and ending in my nervous system. When what I perceive is in my own body, the same is true, except that the chain of physical causes begins at the surface of, or within, my body instead of outside it. In either case, what finally happens in my nervous system at the moment when I perceive either an external object or my body is physically quite unlike the initial change which causes it. There is, for example, no resemblance between the light-waves which originate in the sun or are reflected from a table, and the complex electrical changes which occur in my brain when I see the sun or a table. A sense-datum, therefore, if we use that term to describe a colour, a sound, a touch, or a smell which we experience, is caused by the object which we see, hear, feel or smell, but cannot be either a part of that object or qualitatively like it. Moreover it is argued that, since in perceiving we are dealing with a chain of physical causation which takes time, we must always perceive an object at a time later than that at which the series of physical events which leads to its perception starts from it. This time is only a fraction of a second when we are concerned with awareness of events in our own bodies, or with seeing an object on the surface of the earth, but it may be appreciable when we are dealing with the sound made by a rapidly moving object such as an aeroplane, which sounds as if it were in one place when we can see it in another, or when light travels from some celestial body to the eye. In the case of the sun, for instance, it takes about eight minutes for a light-wave to reach us. Hence it is argued that the only information which sensation can give us must always be about the past state of the object perceived. This makes no practical difference in everyday life because the objects with which we deal change so little from moment to moment, that for practical purposes their present state, at the moment at which we perceive them, is identical with that at which the light-wave or sound-wave left them. In the case of astronomical objects, however, the time factor is of practical importance, and has to be taken into account. If the light-waves from a distant star take several thousand years to reach us, we should not know if it had become extinct several hundred years ago. In what sense, therefore, can we be said to be seeing the star; and must we not conclude from this temporal argument, that once again our sense-data cannot be identical with the object which excites them?

---

* Read to the British Society for the Philosophy of Science, 30.xi.59

This view of the purely causal relationship between object and sense-data leads to the question, where are sense-data situated? Visual sense-data are situated in a space in which they are related to each other, and to the body of the observer. Sounds are on the whole less sharply located; nevertheless sound is in, or comes from, a certain direction in most instances, and the directions from which sounds come can be distinguished from one another. Smells are related to space in a more diffuse way, but they also have spatial reference. Some of my bodily feelings are very precisely located, others more diffusely, but they also are all somewhere.

If we become aware of all these sense-data as the result, and solely as the result, of changes occurring in the brain, which is inside the skull, how do they come to be experienced either outside the body when they are related to external objects, or within the body itself, but never within the brain? Some thinkers, chiefly physiologists and psychologists, have tried to get over this difficulty by invoking a process, which they term projection. Sense-data, though depending upon physiological changes in the brain, are held to be projected to the spatial position in which they seem to be located. No-one has ever succeeded in describing how projection is brought about, and on examination it seems to be no more than an imaginary process invoked in an attempt to describe how changes within the brain lead to an experience in which a sensory quality is located outside the brain.

Some writers have sought to get over this difficulty by supposing that sense-data or perceptual objects exist in one space and physical objects in another. (I do not think it necessary here to discuss the relationship between sense-data and perceptual objects, because I do not think it has been suggested that they might be located in different spaces.) Since Bertrand Russell has been the chief contemporary exponent of this view, I shall illustrate it from his exposition.

> Naïve realism identifies my percepts with physical things; it assumes that the sun of the astronomers is what I see. This involves identifying the spatial relations of my percepts with those of physical things. Many people retain this aspect of naïve realism although they have rejected all the rest. But this identification is indefensible. The spatial relations of physics hold between electrons, protons, neutrons, etc., which we do not perceive; the spatial relations of visual percepts hold between things that we do perceive, and in the last analysis between coloured patches. . . . When I say that something is 'outside' me, there are two different things that I may mean. I may mean that I have a percept which is outside the percept of my body in perceptual space, or I may mean that there is a physical object which is outside my body as a physical object in the space of physics. Generally there is a rough correspondence between these two. The table that I see is outside my body as I see it in perceptual space, and the physical table is outside my physical body in physical space. . . . One of the difficulties which have led to confusion was failure to distinguish between perceptual and physical space. Perceptual space consists of perceptible relations between parts of percepts, whereas physical space consists of inferred relations between inferred physical things. What I see may be outside my percept of my body, but not outside my body as a physical thing. Percepts, considered causally, are between events in afferent nerves (stimulus) and events in efferent nerves (reaction); their location in causal chains is the same as that of certain events in the brain.[1]

It would seem to follow from this (and it is a view which I have myself adopted in the past), that each observer has his own private perceptual space, whereas there is a physical space common to all observers. My private perceptual space is the space in which are located all the sensory characteristics of everything I perceive, that is objects outside my body, and my body itself, and all that I can perceive as happening within it. Every person is believed to have such a perceptual space private to him. This idea has both advantages and disadvantages. One of its advantages is that it provides a habitat for hallucinations, which are private to the observer, and yet appear to exist in spatial relationship with the world of external objects which he perceives. If one observer says that he sees a man walk through a room, in which other people present say they see nothing, this is not difficult to understand if we believe that the sensory qualities of the objects in the room and their spatial relationships are in some sense private to the observer. The hallucinatory appearance of the man, though resulting from some abnormality in his nervous system, is not to be regarded as an event of a different perceptual status from his awareness of the normal occupants of the room. Similarly if a person who has had a limb amputated experiences a phantom limb and says that it feels as though the limb was still there, his imaginary limb is located in a spatial relationship to the rest of his body and may be said to exist in his private perceptual space though it does not exist any longer in physical space.

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[1] Bertrand Russell, *Human Knowledge: its Scope and Limits*, London, 1948, pp. 217-225

There are, however, certain disadvantages in the idea that we each of us possess a private perceptual space. The first problem which it raises is the relationship of these multiple private perceptual spaces to one another and to physical space. Russell clearly believes that my private perceptual space is in my brain. It cannot, however, be in my perceptual brain, which is situated in my perceptual space within my perceptual head. It must therefore be in my physical brain, and that raises the question of the relationship between my perceptual space and physical space. A disadvantage, which seems to me more serious, is the complication which it introduces into the simplest events of everyday life. In order that I may lift a cup of tea to my lips, I must first by sight and touch become aware of the position of the cup as a perceptual object in my perceptual space, in which also my perceptual hand and mouth are situated. I then go through a series to movements in that space guided by my sense-data of touch, pressure, and vision, as the result of which I succeed in bringing the perceptual cup to my perceptual mouth, and I am gratified to find that although all this has taken place in a perceptual world located within my physical brain, I have succeeded in lifting the physical cup with my physical hand to my physical mouth.

The sense-datum theory, and therefore the concept of private perceptual spaces based upon it, has to meet criticism of a more general kind, namely that it is self-contradictory. This criticism, which has been expressed by a number of philosophers, is well summarised by Hirst as follows.

> If we, as minds, never directly perceive material objects but are only directly aware of images or mental representations allegedly caused by them, how do we know that there are any material or physical causes or what their nature is? We cannot look behind the barrier of ideas to see what their causes are like, if they have any. Locke . . . failed to notice that his theory is self-refuting, for its conclusion contradicts the premise assumed in physiology that we do perceive material things such as sense organs and brains.[1]

I believe that the facts of physics and physiology which I have outlined necessitate some form of the sense-datum theory, but I have come to think that it is possible to retain this without the necessity of believing in a multiplicity of private perceptual spaces. I think that a revised sense-datum theory, in the form in which I propose to restate it, may go some way towards reconciling the representationalist view of perception with the views of some at least of the realists.

In an earlier discussion of this question I said:

> If at least what the philosophers call secondary qualities, such as smells, sounds, colours and so on are quite unlike the physical stimuli which give rise to them, then we must regard them as symbols of physical reality and say that the receptive function of the cerebral cortex is to provide us with a symbolical representation of the whole of the external world, not only distinguishing objects by their qualities, but also conveying to us the spatial relationships which exist between them, and at the same time giving us similar symbolical information about our own bodies and their relationship with the external world. All this information, of course, is given us not merely for the sake of pure awareness or contemplation, though that may sometimes be a by-product of it, but in order that we may act; hence it is linked, in ways with which we are familiar, with the motor activities of the brain.[2]

And Russell said:

> Percepts, considered causally, are between events in afferent nerves (stimulus) and events in efferent nerves (reaction); their location in causal chains is the same as that of certain events in the brain.[3]

Both of these accounts recognise the importance of receptive and motor functions and, by implication at least, the dependence of the latter upon the former. Russell goes further in locating percepts at regions of the brain regarded causally as between afferent and efferent pathways. Percepts are thus treated, both as experiences, and in respect of their physiological substrata, as isolable from their motor accompaniments or sequelae.

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[1] R. J. Hirst, 'Perception, Science and Commonsense', *Mind*, 1954, **63**, 388

[2] W. Russell Brain, *Mind, Perception and Science*, London, 1951, p. 4

[3] Russell, op. cit., p. 225

No doubt we habitually consciously isolate the perceptual elements in an experience from our actual or potential actions with regard to them, and this has led to the belief that percepts exist as pure sensory events, and the neurophysiological basis of which must therefore be at the termination of the relevant afferent sensory pathways, and proximal to the neural substratum of any motor activities to which they may give rise. I suggest that both sense-data and percepts are abstractions which, however convenient for many practical purposes, are misleading when we come to consider the nature of experience, either epistemologically or physiologically; that experiences which possess nothing but sensory characteristics, do not exist; that what we mean when we talk about sense-data and percepts is something different; and that, whatever it is, it is not to be found stuffed into a crevice between sensory and motor nerve-pathways in the brain.

If we adopt the sense-datum theory in the form proposed by Russell, the whole of the space which I perceive exists in my physical brain. Hence when I lift a teacup to my lips, the sight of the cup and of my hand, the pressure and weight of the handle on my finger and thumb, the sensation of the movement of my arm, and the subsequent pressure of the cup upon my lips, together with the spatial relations of these various experiences, are all located within my brain. My success in performing the manoeuvre depends upon the fact that the various brain events which cause me to have the relevant experiences represent corresponding events in the physical tea cup and my physical body in such a way that when I experience this series of sensations these physical events are taking place. This leads to the central part of the view being put forward in this paper.

Whatever view different schools of philosophy may take about sense-data, no-one, as far as I know, has denied that in normal, as opposed to hallucinatory perception, such physical events are taking place. Hence when I lift a cup to my lips I know both where the cup is, and where my lips are in physical space. If this be conceded, it follows that our sense-data may give us information about the spatial relationships of external objects both to one another and to our own bodies. This information guides, and is validated by, our motor activities in relation to such external objects. This enables us to dispense with the idea of a multiplicity of private perceptual spaces, one for each observer. As far as the physical world is concerned, there is only one space, in which physical objects, amongst which are our own bodies, exist. The spatial elements in perception give us information about the spatial relationship of bodies in physical space. What is private and subjective about perception is that this information is individual to each one of us, and presents itself in the form of a perspective, to use another of Russell's phrases, centred upon our own bodies.

This view of perception is not inconsistent with the belief that the sensory qualities of objects, colours, sounds, smells, and touches, are the products of the activity of our nervous systems, and qualitatively unlike the objects which are their remote and initiating causes, but the spatial characteristics of the objects I perceive, such as their size, shape, and relative position, the spatial characteristics of my own body, and the spatial relationships between external objects and my body, are the characteristics and relationships of those objects as physical objects. Unlike other sensory qualities, they are not created by my nervous system, the function of which in this respect is merely to give me information, that is, to make me aware of them.

How on this view are we to deal with the criticism derived from the sense-datum theory, that the time factor involved in perception means that what we are aware of can never be the physical object perceived? The answer to this is that if being aware of objects is identical with receiving information about them, the time interval involved in perception does not impair the accuracy of the information as far as normal perception of most terrestrial objects is concerned. When I see an object, even a moving object, in spite of the speed of light and the time taken for the transmission of a nerve impulse from my retina to the visual area of my cerebral cortex, the object really is where I see it. The same is true of an object held in my hand; for the time taken by the sensory nerve-impulses to reach my brain does not render the information about the size, shape and position of the object inaccurate. The test of the motor response can be applied in each case. The information is reliable as a guide to movement in respect of both the distant object and the object grasped. When we come to astronomical objects, the time factor involved in the conduction of light waves may be great enough to make a practical difference. Now, however, we are dealing with information about the object's past, and this becomes inaccurate only if we wrongly suppose that it relates to the object's present situation. Even in such cases, however, we may have to introduce a qualification. When I speak about shading my eyes from the sun, I do not normally mean the astronomers' sun which is by now in a slightly different position from the sun which I see. For practical purposes the sun, as the object of my perception of light and heat, is equivalent to the light and heat waves during the last part of their passage through the atmosphere, and my sensory information about the sun as an object in this sense is accurate. Similarly, when I look at a star which may be situated many millions of light years away, unless I am astronomically sophisticated I shall not even know of the existence of the astronomers' star, but I shall point correctly to the physical object composed of the light waves from the star during the last part of their course before they reach my eye. In these instances again the accuracy of the information is to be measured by that of the motor response, i.e. shading the eyes from the sun and pointing to the star.

Here we have the answer to the objection to the sense-datum theory made by Hirst and others. As I have said elsewhere,

> If we start our description of perceptual knowledge with a subject and an object, we shall rapidly get into difficulties from which there is no escape. The fact with which we must begin is the fact of knowledge, experience, or information, if we use the last word, as I think we must, to imply a receiver as well as something received. If we start with knowledge or experience we start with the subject-object relationship already given. We do not need to ask how we become aware of things outside ourselves because it is with that awareness that we begin. It is easy to fall into the error of supposing that the sense-data generated by the brain must be *entirely* unlike the physical objects which, by acting upon the brain from the outside world, produce them. But, with some important exceptions, this view is mistaken. . . . One of the most important functions of the brain is to provide us with an accurate representation of the spatial structure of the external world as well as of our own bodies. An important part of this structure is the ever-changing relationship between our bodies and their environment: awareness of the externality of what is outside our bodies is therefore given in all ordinary acts of sense perception.[1]

Hence while there may be forms of the sense-datum theory which imply that, in Hirst's words, ' we, as minds, never directly perceive material objects but are only directly aware of images or mental representations allegedly caused by them ', and in respect of which it is legitimate to ask, ' how do we know that there are any material or physical causes, or what their nature is?' this is not true of the form of the theory now proposed. In that we do have direct awareness of external objects, and of our own bodies. We do perceive material things such as sense organs and brains. We can measure distances, infer from our observations the speed of light and of the nerve-impulse, and learn from physical and physiological facts, particularly the structure and activity of the nervous system, that some sensory elements are the products of the nervous system. If our minds had no information concerning material objects, we could never know that material objects were the causes of our sensations: if, however, we have sufficient information concerning the structure of material objects, there is nothing illogical in inferring that some elements in our perceptions are subjective. All that is needed to validate this inference is the power of spatial and temporal discrimination. Whether we discriminate objects such as pointer-readings because they depend upon black and white contrast or contrasting colours may be quite irrelevant to the accuracy of the observation.

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[1] W. Russell Brain, *The Nature of Experience*, 1959, pp. 31-32

It may be said that this does not meet Hirst's objection, for, if our knowledge of the structure of the external world depends upon sense-data which are generated by our brains, and are therefore subjective, we cannot know that there are such things as external objects to cause our sense-data. It is necessary, therefore, to examine the relationship between structure and sense-data. The fact that structure is independent of some sense-data is an everyday experience. We recognise a photograph although it is a flat, black and white representation of a coloured three-dimensional object, and we recognise it as a photograph of the same object whether it is large or small. Similarly we recognise the structure of a word, whether it is printed in black on white or blue on red. The distant hills look small and blue, and as we approach them they become large and green, but we recognise them as the same hills. In order that we may recognise a spatial structure visually, we must be able to discriminate perceptual differences, but within a wide range it does not matter what shades are used to differentiate them, provided the shape of the boundary between them remains the same. Hence, provided an area of the body surface, such as the retina, possesses receptors which are stimulated from different points of space, and the nervous system can discriminate the nervous impulses excited by such stimuli, we possess the basis of visual discrimination of spatial structure in the external world, and the fact that this discrimination reaches consciousness literally coloured by sense-data, which themselves do not exist in the external world, does not mean that the information they mediate is not true information. Most mammals do not possess colour-vision, which is limited to insects, birds, the higher primates, and man. This means that those organisms which do possess colour-vision have evolved a nervous system capable of responding differentially to differences in the wave-length of light. Those without colour-vision can still discriminate objects by vision, but those with colour-vision possess greater powers of discrimination.

Thus we arrive at the view that in perception we have direct knowledge of the size, shape, and spatial disposition of external objects. What we know about them in these respects is, subject to accidental errors, genuine knowledge. They are where we suppose them to be in a public physical space. Our knowledge of this is personal and private, but that does not mean that we create a private perceptual space in which our perceptions of external objects exist. This holds true broadly of terrestrial objects, with which we ordinarily deal. When we come to celestial objects, we must either interpret object as meaning the last phase of the energy they propagate before it reaches our bodies, or we must say that the spatial characteristics of which we have present knowledge are those of their past, and often their very remote past.

To understand the part played by sense-data in perception, it is necessary to look at the problem created for the living organism by the physical stimuli reaching it from external objects, and its need to develop a capacity to respond to them. The physical stimuli are extremely diverse, comprising as they do light-waves, temperature-changes, sound-waves, the presence of particular molecules in the air or in food substances, and the physical changes directly produced by the pressure of objects on the body, the movements of its parts, and gravity. The organism must develop specific receptors which are selectively responsive to each variety of physical stimulus. All that such receptors can do when stimulated is to excite nerve-impulses, which consist of electrical changes in the nerves running from the receptor to the central nervous system. These electrical changes differ somewhat in frequency and rate of conduction, but within a comparatively small range. There is no evidence that any qualitative differences in the nerve-impulses from particular receptors correspond to the very wide range of differences between the physical stimuli. All our awareness of perceptual difference therefore depends upon differences in the spatial and temporal configuration of the electrical changes reaching the brain. When such changes occur, we experience the appropriate sensation, and it is arguable that the occurrence of the electrical changes is the experiencing of the sensation.

It is inevitable therefore that a percept should contain subjective elements, and, as we have seen, it is these which make perceptual discrimination possible. External objects, therefore, are perceived *through* the nervous system, and such of their physical differences as can be distinguished by our sensory receptors are represented by distinguishable sensory qualities of sense-data. We are familiar with the fact that the sensory characteristics of objects are modified by the medium through which light-waves pass on their way from them to us. The colour of distant objects is affected by the state of the atmosphere, and if I look at the world through rose-coloured spectacles, objects will have a rosy appearance. This process of *labelling* physical objects with sensory qualities in order to discriminate their physical properties would be useless if the label were not attached to the object, and this is what happens in perception. We have accurate spatial information concerning the external world, as an inherent part of which it is perceived with sense-data which symbolise some of its physical properties. The same is true of our own bodies.

Thus, although it is sometimes convenient to talk about pure sense-data, and the localisation of sensory pathways and centres in the nervous system, this is an abstraction from a more complex and complete totality. Quite early in infancy the visual appearance of an object stimulates the young child to make movements with its arms in the direction of the object. If it succeeds in grasping it, vision is reinforced by sensations of touch, and knowledge of the direction of the object becomes linked in an elementary way with knowledge of the movements required to grasp it. So perception is from the first sensorimotor, and sense-data through experience may come to convey also other information related to other potential sense-data as of texture, weight, etc.

Much of this may not be obvious even on introspection, but it has at least originated in conscious experience. Physiology shows us that perception is profoundly influenced by factors which do not as such enter consciousness at all. The main evidence for the part played by these unconscious afferent impulses is the profound disorganisation of perception which occurs when they are themselves disorganised or cut off. For example, our whole perceptual orientation in space depends upon the continuous reception of nervous impulses from the labyrinth, yet we are completely unaware of these as such. Hence not all perceptual information is conscious information in the sense that we are aware of it as constituting an individual element in what we perceive, like a colour. Nevertheless it contributes to the totality. This alone illustrates the inadequacy of any philosophical account of perception which is limited to the untutored observations of the percipient, however introspective.

Finally there is a question which is fundamental for our understanding of perception, and indeed of consciousness itself. In 1946 I wrote:

> Let us suppose that we are watching an observer looking at a circle. There is a sense-datum or a group of sense-data which are circular. Light waves, which in their grouping maintain a circular relationship, travel from the object to the eyes of the observer. On a circular area on each of his two retinae disturbances are set up which excite nervous impulses which travel through his optic nerves, tracts and radiations to the visual areas of the cerebral cortex. Only when the nervous impulses reach the cortex does the observer see the circle. If physiological idealism is true we might expect to find that there is something circular about the events at the cerebral cortex, for it is these, we are told, which are ' projected ' on to the outside world when we perceive a circle. Nothing of the sort is true. The area of cortical excitation which exists when we perceive a circle is divided into two halves, one in each cerebral hemisphere. Pathways connecting them exist, but these appear to play no part in our perception of the two halves of a circle as one whole, for this still occurs when the connecting pathway (the corpus callosum) is divided.[1] Neither half is semicircular; it is roughly the shape of ⊃, the closed end lying in front and the open end behind. The right half of the circle is represented in the left cerebral hemisphere and vice versa, and the lower quadrant is represented above the upper. There is another complication. We saw that a circular nervous disturbance occurred in each retina; thus each half of the circle has a double representation in the nervous system, and, though the pathways for the two corresponding halves—that is, the two right halves and the two left halves—come to be close together behind the optic chiasma, and are represented in the same area of the cerebral cortex, there is no anatomical point at which they fuse, as Sherrington[2] demonstrated in

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[1] A. J. Akelaitis, *Arch. Neurol. Psychiat.*, Chicago, 1941, **45**, 288

[2] Sherrington, *The Integrative Action of the Nervous System*, London, 1906, pp. 354 sq.

> his experiments with flicker. Finally, since the two halves of the circle are represented in cortical areas lying parallel to each other, the cortical disturbance is three-dimensional.
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> Thus when we perceive a two-dimensional circle we do so by means of an activity in the brain which is halved, reduplicated, transposed, inverted, distorted, and three-dimensional. If physiological idealism is to be really physiological it must admit that its theory of projection breaks down because the circle which is said to be projected from the cerebral cortex never existed there at all.[1]

To-day I should qualify and extend this statement. The visual cortex is a necessary condition of vision, for its destruction renders the subject blind, but the same is true of the optic nerves. Why, then, do we attach importance to the cortex? Because it is the last anatomical point on the afferent visual pathway of which this is true. But it does not follow that the function of vision is *located* there. It may be inaccurate to speak of its location in any particular part of the nervous system. If the brain largely acts as a whole, vision may be a property of the whole of it, in which case it is represented in the brain as a whole in a different way from its representation in the visual cortex. The latter then becomes only the last point at which nerve-fibres solely concerned with vision exist as an isolable group. The same may be true of other sensory pathways, and of the motor cortex, and motor pathways too. If so, the fact that there is nothing circular about the activity of the visual cortex when we see a circle becomes part of the larger question: is there anything circular about any of the brain's activities in these circumstances? Is neuropsychological isomorphism a necessary concept, or indeed a meaningful one? When we perceive a circle there is always something circular in the body (I am not now discussing tilted circles). If I see a circle there is a circular disturbance on my retinae. If I feel a penny pressed into my palm there is a circular disturbance in the skin of my hand. The information concerning the circularity of the visual or tactile object is conveyed along the relevant nerves in a complex pattern depending upon the particular nerve-fibres excited, the rate and frequency with which impulses pass up them, and the temporal pattern of their individual excitation. In other words it is coded. Up to and including the cerebral cortex there is a detectable correlation between a nerve fibre and the point on the retina or skin excited, as shown by the sensation aroused by stimulating it and the defect caused by its destruction. Beyond this there is no such correlation detectable. Somehow this coding of spatial information in a spatio-temporal pattern of frequencies is decoded into a static representation of a spatial pattern.

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[1] W. Russell Brain, *Philosophy*, 1946, **21**, 133

How this is done goes to the heart of the problem of the brain-mind relationship. We do not yet know enough to describe it in detail, but we can see at least the outline of the process. Consider some simple facts. I ask someone to close his eyes and stretch out his arm, and by moving his extended forefinger to describe a circle in space: next, keeping the forefinger still, but moving the wrist, to describe the same circle, by which I mean a circle in the same place: and, finally, keeping both the finger and wrist still, to do it again by moving the arm at the shoulder. No one has any difficulty in doing this, nor, in doing so, doubts that the three circles are, approximately at least, *in the same place*. What does this involve in psychological and neurological terms? Psychologically there is initially the decision to carry out the movement, which in some people will be accompanied by a visual image of the circle to be described. Neurologically, a succession of motor impulses will cause contractions of the right muscles in the right order to produce the desired effect. For each of the three movements the muscles will be different, and the parts of the motor cortex of the brain excited will differ correspondingly. Psychologically, while the movement is occurring there will be awareness of its circularity, derived (neurologically) from sensory receptors chiefly in and around the joints, which again will be different in each case. The resulting sensory impulses will reach different regions of the sensory cortex and there will be nothing circular about their disposition there. The nervous system, then, must have the means of extracting from these completely diverse sources the conscious information that the tip of the finger is describing a circle in the same place, just as on the motor side it is able to use different groups of muscles to the same end. We may note in passing that to describe one circle involves the retention in memory of the earlier part of the movement as a guide to the rest, and that memory is also required to enable the second and third circles to copy the first. But the process is much more complex than that, for the three successive movements would not describe circles in the same place unless the body had meanwhile remained stationary, so all the sensory and motor pathways and muscles concerned in the maintenance of the posture of the body have played an unconscious part in the procedure. And 'the same place' has meaning only in relation to the rest of the space in which the body and external objects are situated, where the results of the questionnaires were discussed. In its essentials the discussion covered (1) the content and purpose of university courses in the history and philosophy of science, (2) ways and means of increasing and improving teaching in this field in modern universities. Various recommendations were made for stimulating an interest in the subject and these have still to be acted upon.[1]

We may summarise some of the main conclusions of the Leeds conference. It was felt that there was a need to create more specialist posts in the subject. University teachers were needed for (*a*) research, (*b*) undergraduate teaching, (*c*) post-graduate teaching. There was a need to include the history and philosophy of science as part of the general education of sixth-formers as well as undergraduates. There was also a need to provide courses in the departments of education as the demand for the history and philosophy of science in schools will probably expand. A report of the Science Masters Association has stressed the importance of developing teaching in this subject.

As the conference saw it, the introduction of the subject in university curricula has three main functions: (1) the advancement of knowledge; (2) general education; (3) the training of teachers.

Among the more general points made was that few historians turned out by history departments have sufficient interest in and knowledge of science to specialise in this field. The consensus of opinion seemed to be that the history and philosophy of science provides a way of introducing science to arts students that may be more effective than instruction in one science. It gives them opportunities to become acquainted with different modes of thought and for mixing with students of different faculties. The conference also was of the opinion that teachers in the history and philosophy of science need to have a knowledge of both subjects, and that courses in the history of science must either include or be accompanied by instruction in the philosophy of science.

No strong opinions were expressed as to whether the subject was to be taught in a separate department or as part of a larger department. However, as we shall see below, Melbourne in the light of its own experience is strongly in favour of an autonomous department. No doubt the success in the past of the post-graduate department at University College, London, is to be traced to this fact. In a modern university, unless a subject has departmental independence and representation on the governing academic body, its chances of internal growth may be slight. One of the reasons why the subject has not grown commensurately with its educational value, is that it has usually had to be a subsidiary enterprise of one or two interested people who have not always been strongly supported by their own department.

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[1] Professor S. E. Toulmin and his colleagues carried out the Leeds enquiry, whilst Professor P. H. Nowell-Smith reported on the conference.

Our own questionnaire was modelled on the Leeds one. This was done to make our survey comparable with the results of the previous investigation. Our enquiry therefore incorporates the Leeds data as far as modern English universities are concerned. We have also included one or two universities not covered by the Leeds survey.

The Commonwealth universities, with the exception of Melbourne, showed a similar pattern of activity to the British ones. On the whole, though most universities are aware of the importance of the history and philosophy of science, very few independent appointments have been made in this subject. Most of the lecturing therefore tends to be of a stop-gap variety. It is carried out by members of science, philosophy, or education departments working individually or as a team.

Scientists seem more aware of the need for instruction than philosophers, and philosophers more than historians. As a general rule science departments are more inclined to interest themselves in the history rather than the philosophy of science.

Most of the replies stressed the importance of the subject in university curricula. In the case of science students such courses were seen as a means of integrating knowledge of the physical world with the humanities, and as a counter to specialisation. In the case of arts students it was thought that courses of this sort enabled them to obtain a general scientific outlook, and to become conversant with the influence of science on philosophy and culture. It was also suggested that the history of science should form part of post-graduate courses of study in education, philosophy and history. For philosophers history of science was seen to be particularly important as science and philosophy have been so closely inter-related in the past.

Melbourne suggested that there are two distinct functions of a course in the history and philosophy of science, (1) as a discipline in its own right and (2) as allowing an opportunity for inter-faculty communication—bridging the gap between arts and sciences. It also made four further points which are worth quoting since they have some bearing on the development of the subject.

(1) The need for junior courses to be specially designed according to the background of the audience.

(2) The members of the department were all convinced of the benefit accruing from their position as an autonomous department. They felt that they would be at a serious disadvantage if they were a sub-section of History, or Philosophy, or any one of the science departments. They urged the appointment of a professorial head of the department, who would be able to look after the interests of the subject in the Senate.

(3) They were convinced of the need for a group of people with different academic backgrounds to handle the different special sciences. It was erroneous to believe that one person can deal with all branches of the history and philosophy of science.

(4) They were also convinced of the importance of teaching the combination of history and philosophy of science rather than teaching these two disciplines separately.

Another point brought out by our inquiry is that interest in the subject seems partly dependent on the size of the university, its date of foundation, and the priority it gives to cultural questions *vis-à-vis* bread and butter ones in its curricula. It may be that a university has to achieve a certain size and prestige before it can afford the presumed luxury of courses in the history and philosophy of science.

As far as future developments are concerned this survey does bring out the need for establishing a number of new posts in both teaching and research. The newer universities might be encouraged to found chairs in the history and philosophy of science. In those universities where such a course might be impracticable it might perhaps be possible to transform certain existing chairs to ones in this subject. Some thought might also be given to the possibility of instituting an Honours course at one or other of the British universities, as has already been done at Melbourne. With the increasing impact of science and technology on everyday life such courses should become progressively more important.

There is certainly some ground for Melbourne's demand that independent departments should be set up in universities. Otherwise the history and philosophy of science seems fated to remain an ugly duckling in the department which acts as foster parent. The failure so far of the subject to achieve an independent status in most universities, may perhaps arise from the fact that it overlaps a number of different disciplines. The 'establishment' in these disciplines may feel that the development of the history and philosophy of science could only be at the expense of their own subject.

A very valuable field to which teaching in the history and philosophy of science may be extended is the sixth forms of schools. Some schools have already started to give instruction. The Oxford and Cambridge Joint Board and the Cambridge Board now set G.C.E. papers in the history and philosophy of science. We may eventually find ourselves with a generation of young men and women with some school training in this subject which they would be able to continue on a university level.

There would also seem to be important teaching possibilities in the field of extra-mural studies. If the present rate of scientific progress continues, the enlightened man in the street may be expected to show a greater interest in science as a cultural phenomenon. Departments of education will also need to give some instruction to those who are going to teach the history and philosophy of science in schools and elsewhere.

In the case of arts students more could be done in the teaching of the philosophy of the social sciences. Arts students would have much less difficulty in understanding the philosophical foundations of the social sciences than that of the natural sciences. In this way they might attain some appreciation of scientific methodology without needing to have specialised knowledge of, say, physics and chemistry. Although the social sciences have only emerged comparatively recently, the philosophical problems they raise are often of extreme importance.

2 · *Summary of Results*

Great Britain

What emerges from the Leeds enquiry is that there were only four universities where the subject may be said to be firmly established. The oldest department is that of the History and Philosophy of Science at University College, London, established nearly thirty years ago. It is, however, largely a research department with the main emphasis on the history of science. Another established London department is that of Philosophy, Logic, and Scientific Method at the London School of Economics. Philosophy of science is studied there, both at an undergraduate and research level. Philosophical problems concerning the history of science are also dealt with. The Oxford and Cambridge posts in the subject are more recent and are post-Second World War creations. The interest of most of the other British universities, with the exception of Aberdeen, seems only to be of fairly recent date.

In the modern universities, a number of lecturing posts have been established in both the philosophy and history of science, as well as in these subjects separately. Extra-mural posts in each subject have also been established. At other universities the history of science is taught by scientists, philosophers or education department staffs. For some years there has been a Reader in the History and Philosophy of Science at Aberdeen, who has done a great deal to stimulate interest in the subject both in his own University and other Scottish universities. A number of universities offer lectures and seminars to post-graduate, research, or education students.

As far as degrees, diplomas, and certificates are concerned, there is as yet in this country no Honours degree in the subject. The Cambridge certificate in the History and Philosophy of Science can be taken in lieu of Part II of a Tripos, as Part I normally qualifies for the B.A. There has been some discussion in Cambridge as to the institution of an Arts degree in the theory and methods of science. There is at Oxford a diploma in the History and Philosophy of Science. One can also take research degrees, Ph.D. at Cambridge, D.Phil. at Oxford, M.Sc. and Ph.D., at London, and M.Sc. at Leicester. It has sometimes been argued that the subject is one better fitted to be taken at a post-graduate level than an undergraduate level, that some maturity of mind and background in other subjects is necessary before it can properly be studied. Though this may perhaps apply to some extent to the philosophy of science, it is doubtful whether it is true of the history of science.

Australia and New Zealand

The Australian universities seem to be well ahead in this subject. It is not clear whether this is due to Melbourne's example or to other reasons. Certainly some of them are large in size and have a large intake of students. This may have led them to introduce the history and philosophy of science in an attempt to solve problems relating to the integration of the diverse disciplines in the curriculum. Melbourne has an autonomous department with a staff of six and two part-time tutors. At the Australian National University and Canberra University College appointments in the history of science are contemplated. Queensland and Sydney each have a senior lecturer in the history of science, though the Sydney post was unfilled when the questionnaire was returned.

(*contd. on p. 210*)

GREAT BRITAIN

COURSES IN THE HISTORY AND PHILOSOPHY OF

UNIVERSITY	TYPES OF COURSE AND TREATMENT
ABERDEEN	*Philosophy of Science.* One lecture per week for 4th yea scientists, 9 lectures for 5th year medicals. Evening semina for staff and post-graduates. *History of Science.* Weekly course for 3rd year Science 3-lecture introductory course for 1st year Science. 5-lecture course for 1st year medicals. 5-lecture course fo 3rd year Chemistry. Lecture seminars in Dept. of Modern History.
BRISTOL	General B.Sc. 3rd year: 5 lectures on Philosophy o science; 31 Lectures on development of various sciences.
CAMBRIDGE	1. Certificate (Post-Part I) in History and Philosophy o Science. Covers one year full-time instruction in Histor and Philosophy of Science. 5 papers set. 2. Optional half subject in Natural Science Tripos Part I 3 lectures a week and discussion classes. University investigating possibility of a combined Arts an Science course.
DURHAM	
EXETER	Occasional lectures for undergraduates (and potenti teachers). Regular (1 per week) in Education Dept.: outline of History of Science to present day. Course in Scientific Method for science teachers.
HULL	History of Science for post-graduate students, mostl scientists and mathematicians. 19 lectures. Philosophy c Science for undergraduates.
LEEDS	Courses on History of Science: B.A. and B.Sc. mixed class 1st year B.Sc. General and Special subsidiary 2nd year.
LEICESTER	Optional course for all General Arts and General Scienc students, and some Special Science students. 3 lectures pe week: survey from beginnings until technical matter becomes too difficult for a mixed audience. Course for Classic Philosophy students. History of Science in part time 3-yea certificate course for experienced science teachers. Adu education courses.
LIVERPOOL	Courses in History and Philosophy of Science are under discussion, particularly for B.A. General.

SCIENCE IN BRITISH AND COMMONWEALTH UNIVERSITIES

POSTS ESTABLISHED AND EXPECTED APPOINTMENTS	TAUGHT BY	BALANCE BETWEEN HIST. AND PHIL. OF SCIENCE	RESEARCH
eader in History nd Philosophy of cience.	Staff of Department.	Largely History of Science.	—
—	Science and Philosophy Lecturers.	Largely History of Science.	—
Lecturers and 1 As- istant Lecturer.	—	Both.	Ph.D.
ost in Philosophy of cience established	—	—	—
—	Lecturer in Education Dept.	Largely History of Science.	—
—	Education and Philosophy Lecturers.	Both.	—
ost in History and hilosophy of Science.	—	History of Science.	—
ost in History of cience. Post in hilosophy of Science n Philosophy Dept.	—	Largely History of Science.	Two-year M.Sc. course in the History of Science.
—	—	—	—

GREAT BRITAIN

UNIVERSITY	TYPES OF COURSE AND TREATMENT
LONDON SCHOOL OF ECONOMICS	Dept. of Philosophy, Logic and Scientific Method. Scientific Method for Hons. Economics. Philosophy of the Natural and Social Sciences for Joint Degree in Philosophy and Economics.
LONDON UNIVERSITY COLLEGE	Dept. of the History and Philosophy of Science. Courses for M.Sc. and Ph.D.: History of Science to nineteenth century. Philosophy of Science. M.Sc. Exam.: Part I. Three papers in History of Science. One in Philosophy of Science. Part II. Dissertation. Undergraduate teaching to history specialists.
MANCHESTER	Course on History of Science, mainly to physics students. Science graduate seminar on Philosophy of Science. Examinable arts course in Philosophy of Science.
NEWCASTLE	Philosophy and some History of Science for Dip.Ed.
NORTH STAFFS.	History of Science for 1st year students in science and some non-science students. Philosophy of Science for 1st year students. Special subject course for final year Philosophy students.
NOTTINGHAM	General lectures for all students contain one or two per year on History of Science. Part of Extra-Mural Studies.
OXFORD	Optional supplementary subject in Honours School of Natural Science. Possible to offer it in satisfaction of the requirements of Part II, 4th year Chemistry. B.Phil. optional paper on 'Chosen period of Scientific Thought'. Post-graduate Diploma. Lectures to undergraduates on science in the 19th and 20th centuries by group of science lecturers. Subject mainly developed at 4th year and post-graduate levels. Proposed discussion for the introduction of a special subject on the History of Science in the Honours School of Modern History.
READING	Some science departments give short historical introductions to their subjects.
SHEFFIELD	Examinable course for 2nd year Science and 4th year education: General History of Science; individual history of particular sciences; dissertation for all students.
SWANSEA	

POSTS ESTABLISHED AND EXPECTED APPOINTMENTS	TAUGHT BY	BALANCE BETWEEN HIST. AND PHIL. OF SCIENCE	RESEARCH
Professor. 2 Readers. 2 Lecturers.	Staff of Dept.	Largely Philosophy of Science.	Post-Graduate Research in Philosophy of Science.
Professor. 2 Readers. 2 Lecturers. Part-time Lecturers.	Staff of Dept.	Largely History of Science.	Department mainly a research one. All students part-time, except students from abroad.
—	Philosophy Lecturer.	History and Philosophy of Science.	—
—	—	—	—
—	Lecturers in Physics, Chemistry, Philosophy.	Both.	—
Post in Extra-Mural Studies	—	History of Science.	—
Senior Lecturer in History of Science. Reader in Philosophy of Science. Reader in Philosophy of Mathematics.	—	Both.	D.Phil.
—	Members of Dept. concerned.	History of Science.	—
—	Scientists, Extra-Mural Lecturer, 2 Lecturers in Education (both scientists).	History of Science.	Yes.
Post in Philosophy of Science established.	—	—	—

IRELAND

UNIVERSITY	TYPES OF COURSE AND TREATMENT
QUEEN'S UNIVERSITY, BELFAST	Courses for Philosophy Honours Students: History of Dynamics; theories of space and time from Descartes Einstein. Course to 3rd year Arts students in 1960-1.
TRINITY COLLEGE, DUBLIN	Optional course for Hons. B.A. degree in Philosophy on t philosophy of the physical sciences. Arts course for prof sional students: option in History and Philosophy Science at simple level.

AUSTRALIA

CANBERRA UNIVERSITY COLLEGE	
MELBOURNE	Arts Pass 1st year. Arts Hons. 4-year course. Architectu 1st year. B.Sc. (B. course) 2nd or 3rd year students qua fying for a science degree for teachers. B.Sc. Hons. 4th (Chemistry). B.Sc. Pass and Hons. 3rd yr. (Physics). P medical voluntary course, 20 lectures. Medicine Div. 1 12-hr. course on Scientific Method.
AUSTRALIAN NATIONAL UNIVERSITY	
NEW ENGLAND	Was introduced as part of Philosophy course for Arts a Science students but received no support from Science—co fined to logical problems.
NEW SOUTH WALES	Sometimes included in Philosophy curriculum in Huma ties and Social Sciences. Forms part of treatment Scientific Method. Expected to be a required course in ne Arts degree.
QUEENSLAND	2nd year Science students—1 lecture, 2 seminars per wee
SYDNEY	Compulsory additional course for 3rd year Science studen All Science departments are required to help stude to become familiar with the history of their subject.

POSTS ESTABLISHED AND EXPECTED APPOINTMENTS	TAUGHT BY	BALANCE BETWEEN HIST. AND PHIL. OF SCIENCE	RESEARCH
Lecturer in the History and Philosophy of Science (1958).	—	Both.	—
—	—	—	—
Appointment expected.	Will be taught by someone qualified in Science and Philosophy.	History of Science with scientific theory and method.	—
Senior Lecturer in charge. 5 Lecturers and 2 part-time Tutors. Autonomous department in Faculty of Arts	Members of the Department of the History and Philosophy of Science.	Both.	M.A. and Ph.D. Research seminars.
Appointment expected.	—	Appointment will be for both.	—
—	—	—	—
—	Philosophy Lecturers.	Both.	Possibility will be considered.
Senior Lecturer.	—	History of Science.	Hoped to extend course to include this.
Senior Lectureship at present advertised.	—	History of Science.	—

CANADA

UNIVERSITY	TYPES OF COURSE AND TREATMENT
Laval, Quebec	History of Philosophy and Philosophy of Science.
McGill	Course offered in History of Science by member of Phil sophy Dept. Course in History of Medicine.
McMaster	Half-year course in Philosophy of Science open to anyo who has previously taken philosophy. 3 hrs. per wee New course on the fundamentals of Physical Science f non-scientists.
Montreal	Compulsory courses for Science and Engineering studen Course for extension students and B.Sc. General (optiona Development of the pattern of thought in Western Civilis tion.
Mount Allison, Sackville, N.B.	3rd and 4th year course for Arts, Science, Engineeri (compulsory for latter). One lecture per week, but main reading course.
Ottawa	History of Medicine. Basic course in Philosophy of Scien as part of Philosophy course for Science students. Historic survey in each science.
Saint Mary's Halifax	Elective course giving outline of scientific progress a tracing its influence on literature and political thought.
Saskatchewan	Compulsory Philosophy of Science for Honours Philosoph available for all students with science background.
Sir George Williams College, Montreal	Half-course in History of Science: development of science nineteenth century. Full course in Philosophy of Science.
Western Ontario	History of each science subject, especially history of medic science. 3rd and 4th year Science Honours undergraduat Post-graduates.
Assumption University of Windsor, Ontario	2nd year General Arts, Science, Commerce attend Philosop of Science, in which some history is included.

POSTS ESTABLISHED AND EXPECTED APPOINTMENTS	TAUGHT BY	BALANCE BETWEEN HIST. AND PHIL. OF SCIENCE	RESEARCH
Appointment expected.	—	—	—
History of Medicine.	Philosophy Lecturer. Professor of History of Medicine.	—	—
—	Scientists and Philosophers.	Largely Philosophy of Science.	—
—	Teachers competent to fill the needs of the curriculum.	History of Science.	—
—	Dean of Science Faculty (mathematician).	History of Science.	—
—	Scientists and Philosophers.	Both.	—
—	—	History of Science.	—
—	Philosophy Lecturers.	Philosophy of Science.	—
—	Science and Philosophy Lecturers.	Philosophy of Science.	—
Sessional Lecturer in Medical History.	Members of Science Faculty.	History of Science.	Research graduates must have seminar course in History of Science.
Professor of Physics and Philosophy of Science.	Professor.	Philosophy of Science.	One course in problems of history of modern thought for Philosophy graduates.

HONG KONG

UNIVERSITY	TYPES OF COURSE AND TREATMENT
HONG KONG	History of Science (optional) for Dip. Ed.; 4 lectures per year Approved course in History of Science for 1st year B.A., a present not taught because no suitable staff. It is hoped to implement the course in the future.

INDIA

ANNAMALAI	1st and 2nd year Humanities students: development o science from beginnings to present.
DELHI	2nd year B.Sc. Pass and Honours students: developmen of Physical and Biological sciences.
KARNATAK	Optional course for all 3 years of integrated B.A. and B.Sc. Histories of various sciences.
MAHARAJAH SAYAJIRAO OF BARODA	Compulsory course in preparatory 1-year Arts and Commerce: outline of development of science through th ages.
POONA	Scheme of extension lectures has been adopted. Philosoph of Science for M.A. students.
PUNJAB	To be introduced at undergraduate and graduate levels as par of regular courses.
RAJASTHAN	Part of Logic and Scientific Method.
VISVA-BHARATI	History of Science compulsory for undergraduates. Philosophy of Science optional for post-graduates.

HISTORY AND PHILOSOPHY OF SCIENCE

POSTS ESTABLISHED AND EXPECTED APPOINTMENTS	TAUGHT BY	BALANCE BETWEEN HIST. AND PHIL. OF SCIENCE	RESEARCH
Post contemplated in History Dept.	—	History of Science.	—
—	Chemistry Lecturer.	Largely History of Science.	—
Post established but not filled.	Science Lecturers.	History of Science.	—
Appointment expected.	—	Largely History of Science.	—
—	Members of Faculty of Science.	History of Science.	—
—	—	Both.	—
—	Science Lecturers.	History of Science.	—
Appointment expected.	—	—	—
Appointment expected.	Science Lecturers.	Largely History of Science.	—

NEW ZEALAND

UNIVERSITY	TYPES OF COURSE AND TREATMENT
AUCKLAND	Philosophy course includes some reference to foundat of modern science.
CANTERBURY	Subject dealt with independently according to view departmental heads.

PAKISTAN

SIND, HYDERABAD	History and Philosophy of Zoology for post-gradu Principles of General Biology, deals with developmen concepts underlying evolution and genetics. B.Sc. H courses in History of Chemistry and History and Philos of Physics.

SOUTH AFRICA

CAPE TOWN	Honours and post-graduate Science students: Histor Science to seventeenth century; discussion of aspect modern science.
ORANGE FREE STATE	2nd and 3rd year Science students: outline of Cosmol Epistemology, Scientific Method and Theory.
POTCHEFSTROOM	Maths., Physics and Biology students 3rd year: survey showing principles underlying scientific theor research.
PRETORIA	History and Philosophy of each science as part of course.
STELLENBOSCH	History of scientific achievements dealt with in approp places in science courses.
WITWATERSRAND	History of Science started in 1959. 3rd and 4th year B 8 lectures in each of two of students' major subjects. P sophy of Science for B.Sc. and B.Sc. Honours 3rd 2 lectures per week for half-year.

HISTORY AND PHILOSOPHY OF SCIENCE

POSTS ESTABLISHED AND EXPECTED APPOINTMENTS	TAUGHT BY	BALANCE BETWEEN HIST. AND PHIL. OF SCIENCE	RESEARCH
—	—	—	—
—	—	—	—

—	Scientists.	Largely History of Science.	—

—	Scientists.	History of Science.	Course for post-graduate science students.
—	Professors of Mathematics and Botany.	History of Science.	—
—	Scientists.	History of Science.	—
Appointment expected.	Scientists.	Both.	—
—	Scientists.	History of Science.	—
Science Faculty has recommended that a post be established.	Scientists.	Largely History of Science.	—

WEST INDIES, RHODESIA and NYASALAND, NIGERIA

UNIVERSITY	TYPES OF COURSE AND TREATMENT
UNIVERSITY OF WEST INDIES	In the Education department prospective science teachers hav questions in the Philosophy of Science raised with them.
UNIVERSITY COLLEGE OF RHODESIA AND NYASALAND	Some use of the historical approach in the teaching o general courses in Botany, Zoology, Chemistry and Physic
IBADAN, NIGERIA	2 or 3 lectures on History of Science given occasionally i some science departments to Part II Special Honours Student

(Contd. from p. 197)

Apart from its Honours School and research students working for post-graduate degrees, the numbers of which are as yet small, Melbourne seems to act largely as a service department. One feels that the Faculty of Arts at Melbourne must be a very enlightened one, in the first place for setting up an autonomous department, and secondly for agreeing to the introduction of an Honours degree in the subject. It seems clear from the Melbourne experiment that if such a department was started at other universities there would be no shortage of work which could be found for it.

The New Zealand universities do not report very much in the way of activity in this field. They presumably have smaller staffs and a smaller number of students to deal with, and the problem of interdisciplinary integration is probably not so acute.

Canada

Even among the smaller Canadian universities there seems to be some interest in both the philosophy and the history of science. Philosophy of science courses seem to be given as frequently as history of science courses. In some of the university medical schools the history of medicine is offered as a course. There do not, however, seem to be any post-graduate or graduate degrees or diplomas in the history and philosophy of science. Assumption University has a Professor of Physics and the Philosophy of Science. At McGill

POSTS ESTABLISHED AND EXPECTED APPOINTMENTS	TAUGHT BY	BALANCE BETWEEN HIST. AND PHIL. OF SCIENCE	RESEARCH
—	—	—	—
—	—	—	—
—	—	—	—

there is an established post in the History of Medicine. At the University of Western Ontario there is a sessional lecturer in the History of Medical Sciences.

India and Pakistan

Indian and Pakistani universities use the history and philosophy of science mainly for giving science students a historical background and for broadening the interests of arts students. The subject is mainly taught by science and philosophy lecturers. One university, Karnatak, is contemplating a teaching post in the history of science. At Delhi there is a lectureship in the subject, but this remains unfilled. Most of the replies emphasise the need for some historical study of science.

West Indies, Ibadan, Rhodesia and Nyasaland, and Hong Kong

Out of these four widely scattered universities, Hong Kong seems to be the only one which is contemplating giving a separate course in the history of science. Perhaps this is due to its being an older foundation, and, as it were, the meeting point of two great civilisations—Chinese and European.

The University,
Manchester 13

CONDITION, CAUSE, FREE WILL, AND THE DIRECTION OF TIME

H. C. PLAUT

THERE have been repeated attempts to consider the direction of time as determined and—as it were—defined by the concept of causality. This idea can already be found in Kant's *Critique of Pure Reason*, and more recently Ayer[1] and Schrödinger[2] have made use of it. Good[3] also discusses it. Ayer says in this connection: ' Our notion of causality is derived from the experience of human action.' But he does not follow up the idea any further. In his book *Space, Time and Gravitation*, written as early as 1920, Eddington expresses fundamental ideas, which connect free will, causality, and the direction of time as opposed to determinism. He does not, however, deal here with the closely related concept of condition. Eddington's main idea, which I believe to be a fundamental truth, appears to have been overlooked by many later writers, and there has been a certain amount of confusion. In this paper, an attempt will be made to clarify the whole problem, including the concept of condition. I shall try to investigate to what extent the concepts of condition and causation are based on indeterminism, and to ascertain what follows logically from the assumption that personal free will—which I believe to be the only tangible macroscopic manifestation of an indeterministic world—is not an illusion. We shall find that each of these concepts is used in two widely differing ways, one of which refers generally to qualities of classes. It can be defined by symbolic logics and holds in a deterministic as well as in an indeterministic world. The other refers to individual persons and circumstances and does not make sense in a perfectly determined world. It can be defined only by words which already presuppose indeterminism, or by pointing at occurrences actually experienced. We shall further find that from the personal experience of free will—if it is not an illusion—there follows inevitably the existence of physical forces in the sense of an ' impressed force ' and the dynamic character of the inanimate world. Lastly, we shall consider the direction of time and shall find that its definition from either of the two concepts of causality involves a logical circle, but that criteria for it can be found in so-called reversible processes in very much the same way as in irreversible processes. We shall assume, for the time being, that man is free to decide and act. I shall return to this question later.

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[1] A. J. Ayer, *The Problem of Knowledge*, London, 1956, p. 192

[2] E. Schrödinger, *Mind and Matter*, Cambridge, 1958, p. 79

[3] I. J. Good, this *Journal*, 1959, **9**, 307

1 *Causality*

Before we start treating the somewhat complex problem of causality, I shall give an explanation of some of the terms used: The concept 'determinism' can be illustrated by the metaphor of chains of events. There can be four kinds of such 'chains': firstly chains that reach from infinite past to infinite future; secondly chains that start at a definite time, but reach into infinite future; thirdly chains that come from infinite past, but end at a definite time; and lastly chains that are finite in both directions. A single unconnected event can be considered a special case of a finite chain. As the connections created by the laws of Nature are so many-fold and far-reaching that no event is known that has no consequences at all, we can omit the last two categories of chains for the time being. We call a world perfectly deterministic in which only chains of the first kind exist, i.e. such that are infinite in both directions. This does not exclude that they branch off and interconnect. In a partially deterministic world, there are chains of both the first and the second kind. Chains can be started by free will; this is what our axiom postulates. For the present, it does not matter whether it is my own free will or the free will of any other living being. If a chain is started, but not by free will, we attribute its starting to absolute chance. This definition of absolute chance may comprise events like the emission of a particle in radioactive decay or the will of a higher being, or—if only living beings can start a chain—it may define an empty class.

We shall now replace the metaphor of chains by definitions based on the concepts of classical macro-physics.[1] We consider events to be determined if they can be foretold from the condition of the world at a prior instant (boundary conditions) and from the laws of nature which generally take the form of differential equations like those used in Newton's and Maxwell's theories. Events which cannot be foretold in this way, we shall consider as undetermined.

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[1] A more general definition which, if so desired, can be substituted for ours, was given by Northrop (Introduction to W. Heisenberg, *Physics and Philosophy*, London 1958, p. 21).

The concept of causation as opposed to that of condition presupposes that the cause precedes the effect or coincides with it, and that both events occur at least once. This concept can be used in three ways:

(1) *Kinematic Causation:* An event A is said to cause an event B, if A precedes or is coincident with B, and B never occurs otherwise. In this definition it is supposed that there is one class C_A comprising like events of a kind A, and another class C_B comprising like events of a kind B, and that an element of the latter class is always preceded by one of the former. An example of such causation from everyday life is the primitive observation of lightning and thunder. Kinematic causality can be ascertained by comparing different chains or different regions of the same chain. We call this kind of causation kinematic (in contrast to dynamic causation). It could also be found in a film consisting of mere patches of colour without inner connection, e.g. if red were always preceded by blue.

(2) *Initiating Causation:* If we apply the definition given above not to classes of events, but to individual events, we are led to a concept that we shall call initiating causation. Here every event is considered to be the cause of all the succeeding links in the chain. With endless chains, this definition is trivial. With starting chains, however, it puts the first event in a special place, and the others may be characterised by their position in the chain, e.g. their distance from the start. It is irrelevant for our purpose whether we consider free will or absolute chance as the first event, or whether we start counting with the first actual event.

(3) *Dynamic Causation:* This is the popular meaning of causation. It assumes that events have the power to create or to influence later ones. We call it dynamic, because historically it has evidently developed from the application of muscular force. The investigation as to what extent this somewhat vague concept is based on experienced facts, will form an essential part of our further considerations.

The following four examples will further illustrate the concept of causality. Example (*a*) represents kinetic causality, (*b*) is ambiguous, (*c*) illustrates initiating and (*d*) dynamic causality.

(*a*) 'Sunshine causes the snow to melt.' This sentence would hold in a perfectly deterministic as well as in an indeterministic world. In so far as it is confirmed by unreasoned observation, it can be based on kinematic causality.

(*b*) ' The mountaineer was killed by a fall of stones.'—' A stone which hit his head, caused the mountaineer's death.' We can look at this event in two ways: if we assume complete determinism, this example represents an irreversible predetermined course of events. If however we admit the existence of chance (in its absolute sense), we can consider this sequence of events as constituting a newly starting chain. But here we encounter the difficulty that every event is dependent on a large number of circumstances. The choice appears quite arbitrary which of these circumstances is to be considered the cause of a certain event. In the case of the mountaineer, any of the following may be regarded as the cause of his death: the stone that actually hit him; any obstacle in the path of this stone that deflected it towards the man; any other stone which started the last one rolling, beginning with the one that started the avalanche; each step that brought the man nearer the place of his death. The solution of this problem is simple: the concept of causality, as it is seen in this example, is based on the assumption of a ' ceteris paribus '; i.e. we assume that, all other circumstances being equal, the event we call the cause might or might not have occurred. But there is something artificial in this limitation: when can we actually assume that an individual physical event might or might not have occurred, quite independently of the state of the world in other respects? And how can we point to any one of the circumstances which might be regarded as causes, and say: ' This one was due to absolute chance'?

(*c*) ' By offending him, I made him my enemy.' Here I was free to decide whether to utter the offending word or not. The start of the chain of events is quite clearly defined: it started at the place and at the time when I uttered the offending word. The ' ceteris paribus ' can also be clearly defined: it comprises all those circumstances which are in no way either directly or indirectly influenced by my decision. This is of paramount importance. Only in a case where a chain of events is started by an act of will, can its start be witnessed.

(*d*) ' By winding up the chain of a winch, I cause the load to rise.' If I do not make the appropriate movements, the load will not rise. If the chain is too weak, it will break, and the load will not rise either.

These examples lead to the following conclusions:

Kinematic causality is applicable in general cases only. It presupposes the existence of classes or sets, of which the individual cases are elements. This is the sense in which Northrop[1] uses the word 'causation', but not its popular usage. The application of kinematic causation is justified by induction. That it is paradoxical to apply this concept in some individual cases, can be seen from the following sentence: 'My proposal to Miss A. on that day was the cause of our marriage.' It cannot be replaced by the sentence: 'Each time I proposed to her on that day, we married.' Kinematic causation applies in a deterministic and in an indeterministic world.

Initiating and dynamic causation apply in an indeterministic world only. They refer primarily to individual cases, because the beginning of a chain is an individual occurrence. A number of similar special cases, in which initiating or dynamic causation apply, can be united in a class, and for such classes kinematic causation can be used. A sentence which illustrates this procedure, and in which the word 'cause' is used at once for kinematic and initiating causation, is the following: 'Contempt for the lower classes is the cause of revolutions.' This sentence states the starting of millions of chains on the psychological level, and at the same time uses kinematic causation on the historical level, whole nations being the single elements.

The popular concept of causation does not stop at initiating causation, but proceeds to dynamic causation in applications to the physical world. There is the following interesting line of thought: as in the chain of events started by the act of free will only the first link is simultaneous with this act of will or follows it immediately, the successive links further down the chain can hardly be supposed to be created *directly* by this will, and therefore each link in this chain must be supposed to create the following one. Hence events have the same power as free will to create succeeding events. Similar conclusions can also be drawn if we suppose that chains can be started by absolute chance.

These or similar considerations have doubtless led to the formation of the concept. But can it truly be said that dynamic causation really exists? Three objections can be raised: (1) The chain we have been talking about is only a metaphor. The real world is three-dimensional and continuous with innumerable interconnections. (2) The metaphor supposes that the effect is directional, from each link to the next one. But the law of action and reaction tells us that the effect of objects on each other is mutual and symmetrical. (3) In the metaphor, the effect has a direction in time, but in the physical world, in the case of reversible reactions, time can be reversed.

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[1] A more general definition which, if so desired, can be substituted for ours, was given by Northrop (Introduction to W. Heisenberg, *Physics and Philosophy*, London 1958, p. 21).

We answer these objections by referring to example (*d*). We replace the ' chain of events ' by a real chain, consisting of iron links with a definite mass and modulus of elasticity, and assume that the strains produced are reversible (except in the case of breakage). We furthermore assume that the winch is actuated by a handle, and that the weight is fastened to a hook. If we turn the handle, the stretching and movement of the links passes along the chain in a finite time, determined by their mass and modulus of elasticity, and the weight is lifted. This occurs every time we turn the handle, provided the connection holds; never if we do not turn the handle, or if a link is too weak. We conclude by induction that this will always be the case.

The second objection is not valid: there is mutual reaction between the links; but nevertheless, the effect passes along in one direction down the chain. Reactions pass along the chain in the opposite direction and are felt in our hand. Both action and reaction are directed in time. Objection (2) therefore cannot hold.

As to the reversibility of time in the differential equations of mechanics, the position is as follows: this reversibility is a property of our mathematical apparatus. Though extremely interesting in itself, it is only made possible by the fact that the formula does not contain the whole information conveyed by the experiment; it leaves out the actual direction of time. In applying the formula, we put this part of our information back by coupling in the boundary conditions the appropriate direction of velocities with the progressive direction of time.

In example (*d*), the winch need not be actuated by a human hand, but could be driven by a windmill, which starts working when the wind becomes sufficiently strong. The movement passing down the chain and the lifting of the load would be the same. This forces us to the conclusion that dynamic causation also applies in the absence of free will or absolute chance. In human life, there are innumerable cases where our free will initiates a course of events similar to that exemplified by the winch. The concepts of dynamic causation and of physical force are based on the whole of this experience, which creates an enormous number of associations that come into play when these concepts are applied. Everyone knows how to apply them. Not much more can be said. But I do not believe that more can be said about any semi-empirical concept than that is it based on an enormous number of experiments, and that everyone can apply it. If free will is no illusion, neither is the dynamic character of the world. It cannot be recognised by passive observation. It is revealed and indubitably confirmed by innumerable experiments.

2 *Condition*

The concepts of condition and causation are closely related. Both refer to the occurrence or non-occurrence of two events in connection with each other, and the same facts can often be expressed by conditional as well as by causal sentences: 'If the sun shines on the snow, it will melt.' 'If I turn the handle of the winch, the load will be lifted.' (Corresponding to examples (*a*) and (*d*) respectively.)

There is here the same fundamental division as in the case of causality between conditional sentences referring to general classes and those referring to individual events. In the first case, the word 'if' can be replaced by 'whenever'. Conditional sentences of this kind, which correspond to kinematic causality, make sense in a deterministic as well as in an indeterministic world. Conditional sentences of the second kind correspond to initiating causation and make sense only in an indeterministic world.

The concept of condition differs from that of causality in that it does not presuppose a time-order between antecedent and consequent. If A is a sufficient condition of B, B is a necessary condition of A, and vice versa.[1] Furthermore, the concept of condition assumes that the antecedent may possibly not occur, or may at least be imagined not to occur. It always contains an element of uncertainty, which can arise in an indeterministic world from indeterminacy or from lack of knowledge, in a deterministic world from lack of knowledge only.

3 *Logical Structure of Condition and Causation*

In logical terms, our results so far can be summarised as follows:

Necessary condition: non-A entails non-B; class non-A is not empty.

Sufficient condition: A entails B; class non-A is not empty.

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[1] Ayer, op. cit. p. 93

Causation: non-A entails non-B[1]; class B is not empty; B never precedes A in time.

These three statements provide structural relations between events. They are completely general. They may be applied either to a deterministic or to an indeterministic world. We have now to discuss what instances can be substituted for A and B, firstly in an indeterministic, secondly in a deterministic world.

In an indeterministic world, these instances can be actual events—imagined events—possible events. In a deterministic world, there are no possible events that are not actual events, i.e. they always are or have been or will be realised.

Kinematic causation and the corresponding concept of condition are based on the characteristics of classes and on induction. They therefore always require a plurality of instances, which are considered as elements of classes. They refer primarily to classes and can be applied to individual events only by specialisation. They make sense both in an indeterministic and in a deterministic world.

Initiating and dynamic causation and the corresponding form of condition refer to the starting or non-starting of 'chains'. They therefore refer primarily to individual events and can be applied to classes only by generalisation. These concepts refer to possible events, which may or may not be realised, and do not make sense in a deterministic world. The upshot is that a purely deterministic world cannot include initiating and dynamic causation, because these refer to the choice between possible instances, upon the outcome of which depends what is actually realised.

4 *Direction of Time*

It follows from our analysis of kinematic and initiating causality that neither can be used to define the direction of time, if 'definition' is understood in its strictly logical sense, because in either case a logical circle would arise. In the definition of kinematic causation, it was stipulated that the cause precedes the effect. Thus the definition presupposes the temporal relation. In the case of initiating causation the definition is based on the starting of chains. Here too, 'starting' presupposes the direction of time.

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[1] This refers to '*the* cause'; for '*a* cause', A is replaced by a disjunction, one of whose terms is A. For definitions of '*a* cause', based on probability, see Good, op. cit.

Suppose however we abandon the strictly logical attitude, i.e. of defining in general the direction of time, and instead fall back on seeking a criterion that indicates the direction of time in individual cases. To illustrate, consider a sequence of photos of a garden containing a growing plant at various stages of its life. Since we know that growth is irreversible, the time-direction must be from small to large. Hence we can know the correct time-order of the photos. A reversible process might be used just as well: if the sequence of photos contained a scaffolding being erected or dismantled, we could still know the time-order if we were given the initial conditions, i.e. if we knew which picture was taken first. Other similar procedures, whether based on causality or not, can be used as criteria for the direction of time, but only against the background of our knowledge of the meaning of ' before ' and ' after '. None of them enables us to *define* the direction of time, because all of them contain the notion of it already. This is one of the chief points I wish to make.

I might mention that Einstein makes use of a light-signal to define ' before ' and ' after ' at a distant point. This is perfectly legitimate: he supposes that the meaning of ' before ' and ' after ' is known in the immediate proximity of the observer, and he uses it to *define* these concepts *at distant points*. Some writers appear to consider this definition as a proof that the direction of time is determined by causation. Their view that the use of a beam of light brings in causation, may be justified. But they overlook the fact that ' before—after—simultaneous ' at a distant point are logically not identical with the experienced features ' before—after—simultaneous ' in our sensual perception.

Summary

(1) There are two quite different ways in which each of the two concepts ' causation ' and ' condition ' are usually applied. The first is based on properties of classes and derives its justification from experience by induction. It is generally used in science. I have applied the term ' kinematic causality ' to this form of causation. This as well as the corresponding form of condition make sense in an indeterministic as well as in a perfectly deterministic world. Secondly, there is a form of condition and causation which presupposes that chains of events can be started by chance or free will, and that they might or might not occur. I have applied the terms ' initiating ' and

'dynamic causality' to this kind of causation. These as well as the corresponding forms of condition are those normally used in everyday language. In a perfectly deterministic world, they do not make sense. Any other forms of causality and condition have not been dealt with in this paper.

(2) Free will—if it is not an illusion—is the most tangible macroscopic manifestation of indeterminacy, because it can be witnessed within narrow limits of space and time.

(3) From the assumption of personal free will follow inevitably the idea of an impressed force and the dynamic character of the inanimate physical world.

(4) A definition of the direction of time can be based neither on kinematic nor on initiating causality. Both would involve a logical circle.

I do not claim to have disproved determinism. But proving it by kinematic causation can only be an extrapolation. A proof based on initiating or dynamic causation, which presuppose an indeterministic world, could only take the form of a *reductio ad absurdum*. If determinism is true, we should have to discard the concept of dynamic causation.

I have said nothing about atomic statistics and have touched epistemological problems only in the last paragraph of Section 1. Much remains to be said on these points, but after all some limits had to be set to the scope of this paper.

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DISCUSSION

Physics and Chemistry :
Comments on Caldin's View of Chemistry

I have read with interest E. F. Caldin's article 'Theories and the Development of Chemistry' in the *Journal* for November, 1959. As I understand it, Caldin wishes to point out that there is some contrast between the analytical or critical assessment of scientific activity as involving attempts to *refute* theories, and how in fact the practising scientist makes use of theories in his work. Caldin draws his examples mainly from the field of chemistry, and more particularly, physical chemistry, although at times he also makes some mention of fundamental theory in physics. It may be of interest to mention that quite urgent problems of this kind sometimes appear to arise when physicists and chemists are themselves involved together in scientific discussion.

Caldin says: 'The point is that chemists are interested in finding out what goes on in nature, as well as in explaining it', and later continues: 'It would not be correct, however, to represent chemistry as a science where theory is negligible.' There could be little doubt that the first statement must surely apply to physicists at least equally with chemists, but a serious difficulty, and one which is perhaps not always fully appreciated, seems to arise in their differing conceptions of what a *theory* is. I have come across this situation in practice where such topics as semi-conductors are involved. This field attracts quite fundamental physicists, on the one hand, through such disciplines as statistical mechanics, thermodynamics and electron transport phenomena, while on the other hand the problems of bonding between atoms through the crystal and the importance of various crystal structures, a field often referred to as crystal chemistry, naturally involve chemists. The exasperation which not infrequently arises in discussions, or when a physicist may be trying to assess a paper written by a chemist in this field, has seemed to me in the past to boil down essentially to this rather different conception by each of the essence of a theory and its function.

It does appear that on the whole a physicist (or perhaps better a so-called pure physicist, because probably applied physics lies nearer to what Caldin feels a chemist does) tries to *reduce* his theory at all times to as few parameters as possible and is inclined to feel that a theory is a 'respectable' one, though by no means necessarily correct, if in principle it does offer reasonably specific means for its possible refutation. Moreover the physicist will generally arouse irritation amongst fellow physicists if he is not prepared to abandon his theory when it clashes with subsequent experiments. On the other hand it would appear that the chemist regards theories—or perhaps better *his* theories (!)—as far less sacrosanct, and perhaps in extreme cases is prepared to modify them *continually* as each bit of new experimental evidence comes in. The chemist is surely quite right in believing that such theories or 'hypotheses' will often be very valuable in guiding further research programmes; one recalls the phrase: 'A bad theory is better than no theory at all', or as Caldin says: 'If the term "hypothesis" is used broadly enough, so that it covers the guesses that inspire research programmes. . . . . ' On the other hand, when the physicist gets involved in this, I think he frequently gets smitten with a vague, but rather deep, sense of unease, because he finds it difficult to see how a theory in this sense can ever be proved wrong if the chemist, in the sense I have just used, is always prepared to modify his theory to bring it in line with the latest 'stop press' experiments.

Does this simply suggest that one must beware of trying to lump all scientific activities together, or more particularly, that the analyst or philosopher of science must beware of so doing? On the other hand, it suggests to me personally that the practising physicist or chemist himself might occasionally profit by recognising the rather different methods that each may adopt—largely, one presumes, because the chemist is generally prepared to look at and think about much more complicated situations involving many more physical parameters than the physicist is generally prepared to do. At the same time one calls to mind the remark that: 'When the biologist has reduced his problem to chemistry, he says that it is solved; when the chemist reduces his problem to physics, he says it is solved; but how on earth does the physicist know when his problem is solved ?'

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REVIEWS

THE PROBLEMS OF PERCEPTION

I *Introduction*

THE traditional 'philosophical' problems of perception have, in the past, been dealt with by two different groups of people, professional philosophers and neurologists,[1] between whom there has been little liaison. In the last few years, however, certain neurologists such as Brain,[2] Kuhlenbeck,[3] and Smythies,[4] have attempted to reconcile these two fields by developing the traditional neurological approach to perception to a point where such a correlation has become possible and by making surveys of philosophical theories from this position. Recently a philosopher—R. J. Hirst[5] has performed the complementary task of examining the arguments of both other philosophers and neurologists about perception. His aim is to construct a comprehensive theory of perception that will cover all the facts systematically, which will 'answer the questions traditionally and justifiably raised and which will reconcile customary belief, psychological analysis and scientific findings' (p. 16). This work is of great importance for it represents the first attempt by a philosopher to come to grips with *modern* neurological ideas about perception and the mind-brain relation. Most modern philosophers who have written on this subject have paid scant attention to the views of the neurologists. They held that neither the facts nor theories of the neurologists had much bearing on these traditional problems which they felt arose out of a misuse of language or from the purely technical errors of previous philosophers. However Hirst makes it plain that this will no longer do:

> . . . the scientific account of perceiving cannot simply be dismissed as complementary and innocuous to ordinary notions. It claims superiority, and if it is correct it undermines common-sense beliefs and the 'ordinary language' which presupposes them (p. 319).

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[1] Used in an extended sense to include varieties of neurobiologists as well as clinical neurologists.

[2] Sir Russell Brain, *Mind, Perception and Science*, Oxford, 1951, and *The Nature of Experience*, London, 1959

[3] Hartwig Kuhlenbeck, *Brain and Consciousness*, Basel, 1957 and 'The meaning of postulational psycho-physical parallelism' *Brain*, 1958, 81, 588

[4] J. R. Smythies, *Analysis of Perception*, London, 1956

[5] R. J. Hirst, *The Problems of Perception*, London, 1959

> Nor is it [the attempt to construct a comprehensive theory of perception] a shadow battle with pseudo-problems: for as ordinary language is shaped by or reflects common-sense notions about the nature of things, to show that certain perplexities ' dissolve ' when due attention is paid to its logic is simply to show that they do not arise for common sense, untutored and unscientific as that may be. Thus the essential point is missed that the problems of perception are problems largely because a study of the psychology and physiology of perception and of the characteristics of illusions and hallucinations seems to call for a radical revision of the plain man's assumptions (p. 16).

Hirst's main task is to lay out certain assumptions about perception made by commonsense and then to see how and to what extent these assumptions are threatened by neurological facts and theories or by philosophers, such as Broad and Price, in the same general camp as the philosophising neurologists.

2 *The Sense-Datum Theory*

In general we can say that Hirst is much more sympathetic to the representative theory than is the majority of philosophers and his own theory has much in common with this theory. Naturally enough he starts off with a consideration of the sense-datum theory. Here the only point that I wish to make is that most of the difficulties that he finds in the traditional arguments for sense-data (see pp. 26-37) arise from the definitions of ' sense-datum ' given by Price and others in terms of ' knowledge ' or ' certainty ', or on ideas about ' direct apprehension ' or ' what is given to consciousness ', or on premiss-conclusion types of argument such as the argument from differential certainty.[1] However, alternative definitions have been based on such ostensive properties of the visual field as the relation between sense-data and after-images which have been claimed by Moore [2] and Smythies [3] to be observable on introspection. These definitions seem to avoid these difficulties and they enable us to use the sense-datum theory without being forced to engage in perpetual skirmishes about the epistemological soundness of our foundations. Hirst then goes on to examine certain difficulties in the concept of sense-data, the first being the dilemma of attention—the apparent need to assume an unvaryingly excellent awareness in sensing. It is argued that if we define sense-data along lines such as were used by Price in terms of what is certain in perception, then our awareness of sense-data must always be clear cut enough to preclude doubt. This does not seem, however, to tally with the introspective facts and leads to certain awkward logical consequences. Again this difficulty arises from the vulnerable epistemological definition of sense-data and if we accept instead the phenomenological definition I advocate[1] we can then say, without refuting our own premiss, that sense-data can be either clear or fuzzy (as when I take my glasses off) and awareness can be clear or fuzzy too (as when I am drowsy or giddy). I have dealt elsewhere[2] with the further problems that he raises in this section about sense-data—i.e. their location, category, and relation to material things and these points will be commented on later when I discuss my own theory in the light of Hirst's criticisms. He then deals, in the next two chapters, with phenomenalism and with linguistic methods in philosophy and my only comment here, for what it is worth, is to agree with nearly everything that he says.

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[1] See Hirst, op. cit., p. 32

[2] G. E. Moore in *British Philosophy in the Mid-Century* (C. A. Mace, Ed.), London, 1957

[3] Smythies, op. cit.

3 *The Representative Theory*

So we come to the traditional neurological account of perception—the representative theory. Hirst first gives an account of modern statements of the theory as made by Eccles, Brain, and myself, and he then brings arguments against it. However his statement of the theory itself contains a number of slips and his arguments will not bear the weight of refutation that he has placed upon them. I will deal first with the errors that he makes in presenting the theory. He claims that the representative theory is objectionable because it leads to a duplication of perception. If we do not see objects but see only sense-data (percepts) the problem seems to arise how to account for this second internal perception. Why, he asks, if external perception needs brains and sense-organs, does not this internal perception need them too? This seems to lead to absurdities about 'little men in the head' (see, e.g. p. 280) or to a vicious infinite regress. These objections may be answered as follows. His error is to suppose that the representative theory states that we 'see percepts' or have a 'perception of percepts' or that it postulates a 'new perception of sensations'. Modern versions of the theory make it plain that we need not say that we *see* or *perceive* percepts, sensations, or sense-data. We should say that we *have* sensations or percepts or that we *sense*[3] sense-data, and that we *see* or *perceive physical objects* by means of the part-whole relation holding between sensing and perception. That is, the theory states that *sensing* is the final event in any perception—that which takes place in consciousness—and perception includes sensing and, in addition, a whole complex of physical and physiological events *outside* consciousness.

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[1] Smythies, op. cit.

[2] Smythies, op. cit.

[3] We can say 'what you do when you are inspecting (examining, watching, observing) an after-image or eidetic image or mescaline hallucination is sensing and whatever is common between these activities and the perception of a physical object is sensing' (this is of course Moore's definition).

The further development of this point will also answer another of his objections. This is the familiar epistemological criticism of the representative theory ' if you can only observe sense-data how do you know that there are any external physical objects to cause them ? ' This question can only be answered, as Hirst correctly asserts, by the ' best explanation of the world ' hypothesis and by abandoning the impossible ambition of finding a logical proof of the existence of the external world. The further question then arises that, granting the existence of such a world, how could private sense-data mediate any information about the characteristics of such a world ? This may be answered by a proper attention to the logic of representation. It is often assumed that the information content of any set of objects or events always refers primarily to the characteristics of these objects or events, i.e. the fact that an object is round or brown is held to be primarily a fact about that object itself.

In order to develop this point we must first distinguish between *symbolical* and *non-symbolical* representative mechanisms. A representative mechanism is one that transmits information. It may do this either by means of a signal code (symbols) as in the case of the telegraph or semaphore, or without such a code, as in the case of television and the cinema. It is a characteristic feature of the latter type that observation of one set of events gives us direct non-inferential information about another set of events not observed directly (common-sensical ' directly ') at the time. The representative theory of perception holds that, in the sense that the pictures on the television screen are non-symbolical representations of what is going on in the studio, sense-data are non-symbolical representations in consciousness of what is going on in the external world. Hence it would not be correct to talk about sense-data as *symbols* of their external causes as Hirst does on page 279. Philosophers always seem uneasy when television is advanced as a model of certain aspects of perception. However the word *is* tele*vision* and when we watch television we gain information about the events televised, although we only observe (strictly) a pattern made by lines of light on a screen. No process of inference, deduction, or decoding is involved and nor does what we *see* depend upon any suppressed inferences derived from our knowledge of how television works. A savage watching television would *see* lions and warriors and spears (although magical *explanations* of this unusual seeing would doubtless be advanced on a closer inspection of the situation, the mode of his explanation would not alter the fact that he was seeing the lions, etc.) Very small children accept television completely without knowing how it works. Therefore television provides a sound model of the relation of sensing to perception from an epistemological viewpoint, and shows us how sense-data can mediate facts about the external world.

The adoption of this model leads, however, to the difficulties concerning the further explanation of the processes involved that Hirst brings forward.

He says that perception needs eyes and brains and as we need eyes and brains to see the picture on the T.V. screen, therefore we ought to have 'internal' eyes and brains to see the sense-data themselves—which seems ridiculous both in itself and because it leads to an infinite regress.

The psychological reasons for bringing forward this argument seem to me to be as follows. We have for so long considered that sensing is the whole of perception (as a simple direct acquaintance with objects) and to contain within itself the whole causal chain of perception that, if we consider that the representative theory states that sensing is only the final outcome of this causal chain, we feel that, if this is so, then there must be another ghostly causal chain to replace the physiological chain that has been excluded [1] from the reach of introspection. This feeling is, however, merely an irrational hangover from our old habits of thought, for there is no reason why *perception* should not require an elaborate physiological apparatus (to conduct these processes of perception that take place outside the field of consciousness to consciousness) yet *sensing*—the immediate prehension of sense-data available to introspection—should require none. It may merely be the property or activity of the Pure Ego—'that which in us perceives and thinks'—not needing any such apparatus.

The complete account would run as follows. There is perception of physical objects and a part of this is composed of the physiological causal chain of events that culminates in the construction of a sensory field. The other part is the final (non-physiological) event of this chain—the sensing of this sense-field by a Pure Ego.

There is no reason why this final event should in turn be divisible into another causal chain, nor why explanations found to hold in the case of perception *must* also fit sensing, nor can we give any further explanation of sensing except to say that it occurs and that our evidence that it does so is introspective. However, any causal explanation soon reduces to terms that are ultimate for which no explanations can in turn be found, so this is no embarrassment. To explain an event is to find within it further links in the causal chain or it is to demonstrate hitherto unknown external relations to other events. Sensing cannot, on the introspective evidence, be subdivided and it relates only a Pure Ego to sense-data and sensory images. The brain events correlated with sensing can of course be (and are being) elucidated but these are external relations of the process. To suppose that sensing must be divisible into a causal chain is to apply irrelevant criteria of explanation.

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[1] This exclusion arose because representative theorists claimed that the events of the physiological causal chain occur both temporally prior to the construction of the final percept and that they take place out of the reach of introspection: see in this regard the accounts given by Fessard and Lashley in the recent symposium on Brain Mechanisms and Consciousness (J. F. Delafresnaye, ed., Oxford, 1954) and my discussion of this symposium in *Brain* ('Brain Mechanisms and Logic', 1957, 80, 393).

The logical point can be generalised. We may consider that we are dealing with a set of events A with a set of characteristics X. It is then postulated that the set A really consists of two subsets *a* and *a′* and that most people have previously misidentified A with *a*. It is further suggested that some of these characteristics X belong to *a* (as *x*) and some to *a′* (as *x′*). We then have no cause to insist that *x′* must characterise *a*, and we must realise that our compulsion to do so arises from the strength of our previous undoubting but yet erroneous identification of *a* with A. To apply this argument in this case substitute for A ' the events involved in perceiving an object ', for *a* ' those parts of A available to introspection ', and for *a′* ' those parts of A not so available '. X, *x* and *x′* are merely the properties of A, *a* and *a′* respectively and in this case only *x′* needs to be specified, i.e. ' forming a physiological causal chain '.

Hence the modern representative theory neither duplicates perception nor is there any reason to accept Hirst's claim that we must press the T.V. analogy to account for sensing too. Nor does the representative theory accuse ' us all of ineradicable and inexplicable error ' (p. 179). We certainly do see objects and perceive the events of the external world and the representative theory does not have to say that we do not. Normal perception includes sensing as part but of course sensing can also take place without perception (as in hallucination). Furthermore normal everyday perception and sensing are associated with a particular psychological set and content of thought (in terms of evaluation of and action directed towards the external world). However, even when we are looking at everyday physical objects we can adopt what Quinton[1] terms the phenomenological frame of mind (if we are philosophers trying to discover sense-data, or psychologists interested in constancy). All that is different here is that we adopt a different psychological set and content of thought (directed towards a phenomenological analysis of the visual field). In other words our thoughts and actions are different in each case.

One last point in this section: Hirst asks (p. 174) ' . . . what is the evidence for this novel mental seeing ?' Common usage gives us one. So expressive a phrase as ' the mind's eye ' could hardly have the widespread use it does if there was not something very like seeing about having sensory mental images (I use this term to include eidetic, hypnagogic, and mescaline images).

4 *Mr Hirst's Aspect Theory of Perception*

So all the reasons given for rejecting the representative theory arise either out of Hirst's mistakes in presenting the theory, out of the fact that his conclusions do not follow from his premisses, because he confuses the logic of symbolism with the logic of representation and because he presses the 'television' analogy of perception too far. This considerably weakens his motives for constructing a rival theory but I will now examine this on its own merits. The central notion of his theory is that of 'aspect'. Its use is introduced by six analogies each of which illustrates that those characteristics of an object or situation that we can observe at any one time depend on our point of view and even more so on our mode of access to the object. The simpler analogy is seeing two sides of a building. Modes of access are illustrated by hearing and seeing an accident or more closely by witnessing an accident and actually being in one yourself. A further example is the difference between thinking of a ray of red light and of a train of waves at 700 mμ. The point of using 'aspect' is 'to indicate the limitations of each mode of access, namely that on it alone one cannot be aware of or ascertain all the characteristics of the thing' (p. 189). But, he goes on to say, we must take care not to reify aspects (this is alleged to be the main error of the sense-datum theory) and he emphasises that to be aware of an aspect of a thing is to be aware of that thing itself. This notion is then applied to perception as follows. In perception there are three modes of access to the events taking place. The *inner aspect* is the aspect of the percipient himself—the content of consciousness when we perceive a physical object (p. 220, line 12). The *outer aspect* is the physical and physiological causal chain terminating in a certain brain state. The *third aspect* is taken up by the philosopher who seeks to correlate the inner and outer aspects. From this latter aspect the other two are 'identical in the general sense that they are the one relation or situation of perceiving viewed in different ways or as revealed on two different modes of access': (p. 286).

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[1] A. M. Quinton, ' The Problem of Perception ', *Mind*, 1955, 64, 28

Hirst insists that we do perceive physical objects and he wholly rejects the sense-datum account of private percepts. The relationship between the brain events in perception and consciousness is dealt with as follows. One finds that 'a given content of perception' is closely linked with certain brain activity which is its necessary condition. But this should not lead us to suppose that there is a 'strict identity' between the outer and inner aspect of perception. The content of perception is the whole inner aspect, but the brain activity is only a part of the outer aspect (the rest being causal chains emanating from a physical object). This consideration leads Hirst to postulate 'perceptual consciousness', which is itself a whole with two modes of access. Its inner aspect is the 'same in character' as the inner aspect of perceiving but its outer aspect is brain activity and is thus only a part of the outer aspect of perceiving. The inner and outer aspects of perceptual consciousness can, according to Hirst, be strictly identified in a particularising sense, unlike the inner and outer aspects of perception that can only be identified in the very general sense in which aspects are identifiable and not in any particularising sense.

This scheme may be commented upon as follows. The distinction between identity in only a very general sense based on aspects and identity in a strict particularising sense is going to lead to trouble. The whole point of introducing aspects was that the events of which they are aspects are identical. There is strictly only one object or set of events or situation which present two aspects. Hirst defines the outer aspect of the perception of an object as identical with the whole causal chain (p. 288). But he identifies its inner aspect only with a part of this causal chain, the brain activity. The clause he brings in to save the situation is that the inner and outer aspects of perception are identical only in the sense in which aspects are identifiable. However, the difficulty here is not merely that two aspects of one set of events are different but that the two alleged aspects contain different events—brain events only in one case and brain plus external physical events in the other. Hirst holds that the inner aspect of perception is identical with the inner aspect of perceptual consciousness (in veridical perception) (p. 220 'Perceptual consciousness is normally part of perceiving . . .'). The outer aspect of perceptual consciousness is certain brain events. The outer aspect of perception comprises the same brain events plus other physical events external to the body. Yet Hirst supposes that two aspects of one set of events or of one situation are merely two views of one and the same set of events. Clearly something has to give way here. Two aspects of one object, set of events, or situation can of course be said to contain different events as in the case of two views of one building from the north and south or in the sense that California and Maine are aspects of America.

There are thus two uses of 'aspect' that Hirst does not distinguish very clearly, i.e. two aspects of (rigidly) one set of events or object (such as two views of one surface of a penny) and two aspects that are two different sets of events united only by being parts of some whole (such as the examples given above). The relationship between the inner and outer aspects of perceptual consciousness uses 'aspect' in the first sense and that between the inner and outer aspect of perception uses 'aspect' in the second sense. However, only the use of 'aspect' in the first sense will support the claim that physical objects are directly given in experience (which requires the strict sense of identity between perceptual object and physical object and thus that the inner and outer aspects of perception should be strictly identical): the use of 'aspect' in the second sense will only support the claim that there is some classification in which the content of a perception and the physical object may be regarded as parts of a common whole. But this claim is so weak as to be satisfied by almost any theory of perception and Hirst's whole elaborate theory is stultified. Hirst's use of the notion of modes of access is discussed further below.

5 *Further Criticism of Hirst's Thesis*

There are some further points to discuss about his account of perception.

(4.1) The three modes of access seem somewhat artificial. Admittedly I alone can have my experiences and my nerve impulses are public to anyone with a drill, electrodes, and an oscilloscope. Yet I can observe the nerve impulses in my own brain in a public fashion (under a local anaesthetic such as is commonly used in neurosurgery) and I can certainly do my own philosophising. Therefore the epistemological basis of the three modes of access that is so important for his theory seems much less clearcut than Hirst makes out. There seems to be a further confusion here over 'points of view'. At first Hirst uses this to explain why the contents of my consciousness should appear so different from the state of my brain with which they are supposed to be identical. The pill is made easier to swallow by the examples that show how things can look different from different points of view. 'Point of view' here implies actual perception. However Hirst soon slides into another sense of 'point of view' where it means 'opinion' or 'as considered by a person with special interests and training'. Clearly the third or correlator's 'point of view' is of the latter kind (although the switch is made without any warning of the very different logic of the two meanings). Hirst uses 'aspect' both to cover the original phenomenological introduction of perceptual consciousness (as a normal component of every perception) and as 'a theoretical notion, something conceived from that third point of view . . .'. Yet it hardly seems plausible to maintain that, when I am perceptually conscious of some sensory image, all this is the theoretical notion of some hypothetical correlator. However Hirst gets round this by 'points of view' again. My perceptual consciousness is act/object in character *to me* (on its inner aspect) but from the correlator's point of view it is 'adverbial'.

This distinction is based, however, upon an error of exposition. Hirst states that '. . . the whole point of distinguishing perceptual consciousness from perception is that it is supposed to be capable of occurring without causation by an external object' (p. 289). His whole argument depends on this point—that epistemological considerations about hallucinations are the *only* reasons for distinguishing between perceptual consciousness and perception. But on page 287 he gives *two* reasons for distinguishing between perceptual consciousness and perception—this one and another concerning the disparity in space and time between our percepts and the objects of which they are percepts, arising out of the finite velocity of light. In this latter case he is forced to introduce *objects* of perception (thinly disguised as 'a given content of perception' (p. 286)) that are on *our* side of the spatio-temporal gap between us and physical objects.

In any case the whole notion of an adverbial experience seems fantastic. This is the notion that I do not experience, e.g. red eidetic images but that

I am merely experiencing redly ! It also seems odd that the same event can both be adverbial and act/object in character (which by any ordinary criteria are mutually exclusive) no matter how we juggle our modes of access. If one has a red round after-image it seems strange to claim that one is merely experiencing (no *thing*) redly, roundly, and presumably ' somewhat-at-a-distancely ' instead of saying simply that one has (can introspect, observe, etc.) a red round after-image set somewhat at a distance. We cannot fall back on the concept of ' projection ' of images as Hirst shows symptoms of doing (p. 38) since this has been shown to be a myth.[1]

His distinction between adverbial and act/object modes of experience is based not only upon this error of exposition but also upon a circular argument—to wit : Hirst seeks to prove that perceptual consciousness is act/object in character from the percipient's viewpoint but is adverbial from the correlator's by saying this is so because

(i) no distinction can be made between act and object from outside: for

(ii) the whole point (*sic*) of distinguishing perceptual consciousness from perception is to allow for the experience of hallucinations :

(iii) in hallucinations there cannot be any true act/object relation because no true object is present:

(iv) this is so because the postulation of the existence of any ' true ' objects such as sense-data presupposes the evidence and point of view available only to the correlator because

(v) as soon as one considers physiological processes or attempts to determine the existence of anything one leaves the inner aspect on which alone perceptual consciousness is act/object in character and one must therefore reassess it and take up the correlator's viewpoint—which is

(vi) that perceptual consciousness is adverbial in character—thus completing the circular ' proof '.

(4.2) The ordinary notion of ' aspect ' or ' point of view ' presupposes some observer (even if only a mechanism such as a camera and even if only a hypothetical one) at the nodal point of the aspect. In the case of Hirst's ' outer ' aspect the observer is clearly the person of the physiologist. But what is supposed to fill this role in the case of the ' inner ' aspect? Hirst cannot on his own admissions accept any such entity as a Self or Pure Ego. Yet to what else could the word ' their ' refer (on p. 216, line 24), where the ostensive reference is to human beings who are attributed with ' privileged access ' to their own thinkings (*sic*). The concept of ' privileged access logically presupposes the existence of some entity to exercise this access to thoughts, i.e. an Ego. Does, then, one part of the brain view the other parts? Or do the physiological mechanisms of final sensory analysis and attention merely constitute the outer aspect of what appears on the inner aspect as an awareness of sensations and images? But this does not dispose of the point that the logic of aspect, even of 'inner aspect', requires some x for the aspect to be an aspect *to*. The nature of this x remains very obscure and one is left with the uneasy feeling that Hirst is trying to have his cake and eat it by strenuously denying the existence of a Pure Ego and yet putting the fellow to work in his theory under a number of disguises.

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[1] J. R. Smythies, ' Analysis of Projection ', this *Journal*, 1954, 5, 120

The reason why Hirst introduces these clumsy and dubious 'aspects' is to try to maintain a self-contradictory position—i.e. that the content of our consciousness and the events in our brains have quite different characteristics and are yet identical.

However, even if we allow his doctrine of aspects and agree to overlook the awkward point about to whom or to what the 'inner aspect' of our perceptions are directed (views are views *to* or *from*, as well as views *of*), this reprieve is soon revoked by another major technical flaw in his account. Even if we grant that the inner and outer modes of access to perception will mean a great difference in the characteristics of the aspects revealed one cannot assume that this difference is limitless in range. Certain differences between what are apparently two sets of events may be due to the fact that they are really only two different aspects of one set of events but other differences clearly cannot be so. There are limitations in this that one must explore more closely than Hirst has done. One of these limits is set by topology. Hirst's reply to the point made by Brain and myself—that there is nothing circular about the events in my brain when I am perceptually conscious of a circle—is that identity 'only presupposes congruence when events are arranged in the *same* overall spatial order or frame of reference' (p. 301). He then goes on to claim that what is 'physically one and the same object or event may have a position and dimensions in two different spatial orders, and there will then be no congruence between the shapes and dimensions in the one and those in the other, though there may well be a correspondence' (p. 301). An object may of course have different positions and dimensions in two different spatial orders or frames of reference but it will not have two *completely* different shapes. If we choose sufficiently peculiar frames of reference we can turn a circle into an ellipse or into a number of other closed curves. But it seems to me that mere juggling with our frames of reference cannot change a circle into a member of a topologically distinct species of curve such as an open curve, ring or even two circles without laying us open to the objection that we were no longer presenting an aspect of *one circle* even granted the utmost latitude in our interpretation of the meaning of 'aspect'. In the case of our percepts and the brain states correlated with them, the transformation (as Brain graphically describes) is from *two* neural schemata shaped as inverted and distorted semi-circles (open curves) in widely separated parts of the brain to the one single circle in consciousness. Yet these two are alleged to be identical. The representative theory, as I have constructed it, does not have to wrestle with such difficult problems, for the percept and the brain state are held to be *different* sets of events and so they can be quite different shapes since the only relation between them is causal (or, in my Theory II, *external* spatial as well).

There is a further difficulty in Hirst's theory. To illustrate his point Hirst uses the example of a penny that looks round from one place and elliptical from another. Thus two aspects of one object need not be congruent. However, although the penny *looks* round from one place and *looks* elliptical from another, it is also true to say that it *is* round (determined by repeated scientific measurements in the manner that Hirst describes (p. 166)). Now the brain events concerned in a perception (or in perceptual consciousness) are *x*-shaped and the content of the perception is *y*-shaped. What then is the shape of the one set of events of which they are both aspects? It cannot be both *x*-shaped and *y*-shaped. Therefore it must itself be either *x*-shaped or *y*-shaped or even quite another shape (the penny can really be round yet it can present two aspects that are different ellipses, or even an ellipse and a rod-shape, as when it is seen on end), or perhaps it does not have any shape at all. Hirst's criterion for settling the real shape of an object is by scientific measurement (p. 166–169). Applied in this instance it would give us the answer *x*-shaped. This would, however, give the content of our consciousness the epistemological status of a mere visual illusion.[1] This brings to light another complexity in Hirst's theory and that is the problem of the real nature of the events of which both the brain states as described by the physiologist and the experienced events introspected by the percipient are supposed to be but aspects. The only clue here is that Hirst says that it is perceiving itself that presents these two aspects. Perceiving is defined (p. 285) as a *relation* between a person and other public objects in which he is aware of them and some of their characteristics. But perceiving as a relation could hardly be said to have any *shape*. Yet its aspects have shapes. But perhaps we are pressing the penny analogy too far. The penny presents two aspects that have shapes and the penny has a shape itself, but perhaps perception presents aspects that have shapes yet it has no shape itself. Clearly the confusion arises out of the use of the two different meanings of 'aspect' described above—the phenomenological use and its use to set limits of discourse. However, since the penny analogy depends on the use of 'aspect' in the first sense and Hirst's account of perception rests on the use of 'aspect' in its second sense, it is not at all clear that this analogy can be used to illustrate positions that have arisen in this latter analysis based on the use of quite a different sense of 'aspect'. That is to say, the difference between a brain state and a content of consciousness might *appear* to be like that between that of the two views of the penny but the logic of 'aspect' is quite different in each case and this difference is brought to light by asking of what are our brain states and the content of our perceptions supposed to be aspects. The same problem arises in the case of perceptual consciousness which is defined as 'a whole activity of the person presenting aspects on the two main modes of access' (p. 287). Its aspects have shapes but it apparently has none being a 'whole activity' of the person.

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[1] i.e. a distortion and not an illusion (= illusory) in Hirst's sense (p. 215, line 15) for the *apparent* shape in consciousness is not the *real* shape of the events concerned.

What is going on here seems to be something like this. Let us consider the Stock Exchange. This presents various aspects. It presents two types of internal aspect—what its rooms and corridors look like from inside, and what it is like to be a member of the Stock Exchange, or 'aspect' might refer to details of its internal organisation. Outer aspects of the Stock Exchange can also mean either what it looks like from the various streets around it or what impact or impression the functional organisation of the Stock Exchange has upon a body like the Treasury or the Bank of England. Clearly, if we use the one word 'aspect', or its close relative 'point of view', to cover all these different components with their widely varying logic, the results of our researches will not be as clear as they might have been. In the terms of our analogy of the Stock Exchange, the representative theorist would like to say that the problem arises because the arrangement of the rooms and furniture is not the same as seen from these rooms themselves as it is when we look in through the windows from the street outside. Hirst's method is to say that the Exchange is a whole activity of an organism that presents aspects. But the aspects that he describes, i.e. its aspects to the Treasury and to its own members are not those proper to this context—which are, on account of the spatio-temporal relations involved in perception, its appearance from the street and from inside its own rooms.

6 *Conclusion*

Thus in conclusion one can state that Hirst's arguments against the representative theory, although most thorough and detailed and exhibiting a sophisticated acquaintance with modern neurology and psychology, are invalid. They may be answered as follows:

(i) '. . . the notorious and unsolved problem of how brain activity can cause something so radically different as an experience.' This should be put 'The brain activity causes changes in the content of our experience (in our sensory and image fields)'. There is no *a priori* reason why there should not be causal relations between events in physical space and events in sensible space as postulated by Price[1] and by myself. The idea may be novel but where is the paradox? At least Hirst does not produce one in discussing this particular point (p. 179, line 1) where he claims the idea is a 'paradoxical novelty'. He only says that all scientifically recognised causal relations hold between entities in the same spatio-temporal system. However no *radically* new scientific concept is recognised at the time of its conception. It usually takes a very long time even if it is any good. The previous difficulties lay in trying to imagine causal relations between extended and unextended entities. Another lay in trying to think of the brain churning out images and sensations from its own mechanism. My theory involves neither of these two difficulties.

(ii) The claim that the representative theory duplicates perception is based on a mis-statement of the theory, at least in its most recent form, and the further claim that it leads to ideas about little men in our heads or to an infinite regress arises out of pressing an analogy too far.

(iii) The claim that it is weak to postulate a host of private entities may be met by pointing out that the question of whether there are any private mental entities or not is a question of fact (as Hirst himself recognises on p. 117). The first business of science is natural history—to make catalogues of whatever particular existents there may be and at this point questions of logical elegance do not arise, however important they may be at later stages of the scientific game. Furthermore on his own theory he needs three modes of access to perception, one of which is subdivided into two modes of awareness (p. 190, lines 21-22) and only two of which have any evident correlation with the facts of the matter. He also needs two modes of identity, the slippery distinction between perception and perceptual consciousness, two meanings of 'access' and, moreover, sense-data keep creeping in clad in various disguises such as 'a given content of perception' (p. 286) or 'the content of perceptual consciousness' (p. 295). This complicated theory needs to be set against the simplicity of the representative theory which describes only sensing and perception standing in part-whole relation and their respective objects—sense-data and physical objects. Hirst's first two 'modes of access' can be derived from a consideration of the spatial relations between sense-data and physical objects and they are not used as basic coinage of explanation. His third mode is abolished altogether. The concept of 'access' is not used and only one manner of being identical is recognised.

I do not think it any exaggeration to say that this is the most important book on perception written by a philosopher since Price's *Perception*. Its critical portions should do much to close some overdue philosophical accounts and its constructive arguments should set the form of philosophical debate in this field for the next decade.

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[1] H. H. Price, 'Survival and the Idea of "Another World"', *Proc. Soc. Psychical Research*, 1953, 50, 1

I am most grateful to Professor Sir Aubrey Lewis for his interest in this paper.

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WITTGENSTEIN'S THEORY AND PRACTICE OF PHILOSOPHY

A most striking fact about these two books[1] is this: many people (including myself) are keenly disappointed by reading them, while dipping into them gives great pleasure. I think this split remains even if one both discounts the aesthetic pleasure of lively and forceful prose and remembers that these books are preliminary studies, intended for limited circulation. Another thing is that they make a vastly superior impression, at least on me, compared with Wittgenstein's remarks on mathematics.[2] This too remains after one takes into account that the latter were never meant (by him) even for limited circulation.

In other words, many people do not get from the books what they are led to believe from dipping into them. Wittgenstein himself repeatedly and quite explicitly states what he has to offer (or, at least, what his aims are) in often quoted sayings about letting flies out of fly bottles, etc. Below I shall not try to relate the content of the books to these aims, because I think they are bad ones. Instead, after trying to convey something of the first impression, I shall try to see what emerges from these books for philosophy as a discipline in its own right, or, better, as several such disciplines. I do not believe that the foundations for such a discipline are yet laid, and hence much of what I have to say may well turn out to be short-sighted.

Clarification of Concepts. The book contains descriptions of the use of (ordinary) language in sharply circumscribed circumstances, the so-called language games, and other imagined situations (*Gedankenexperimente*). Many of them illustrate vividly (i) possibilities that may easily be overlooked when one hears for the first time traditional philosophical problems and views or (ii) associations which they perhaps unconsciously, arouse. Before one is surfeited by the torrents of examples, the impression is surely this:

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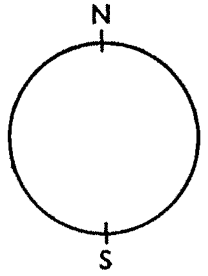
[1] Ludwig Wittgenstein, *The Blue and Brown Books*, Basil Blackwell, Oxford, 1958. Pp. xiv + 185. 25s.

[2] *Remarks on the Foundations of Mathematics.* The review in this *Journal*, 1958, 9 will be referred to as *RFM.*

Here we have a *whole* subject comparable to a development of physics that consisted of nothing but its epoch-making highlights like Einstein's considerations on the measurement of time or Heisenberg's on the simultaneous measurement of position and velocity. No dreary experiments with safeguards against artifacts, no out-of-the-way information for which one needs an elaborate technique, no formal apparatus with paralysing definitions.

It seems to me that much of this would also interest an educated person without special interest in the traditional problems of philosophy such as the nature of our logical concepts or in the reductionist theses of positivism and nominalism. In particular, he will be impressed, or even overimpressed, by the shock effect of unexpected situations. This effect, from which many of Wittgenstein's examples derive a good deal of their force, is, of course, familiar. It seems instructive to consider it by taking a hackneyed example.



Suppose someone (in England) objects that the earth can't be round because then the Australians would fall into space. Wittgenstein would turn the page upside down and say: now *we* fall into space.

The argument might, I suppose, be called philosophical (what else?) where philosophy acts as a handmaiden who does the clearing up in other fields of study, in contrast to being a subject in its own right. This is surely an example of what he had in mind when saying that philosophy clears up misconceptions as one clears away rubbish before putting up a building: in this case something is indeed to be put up, namely the concept of a round earth.

But, more important, the example illustrates how he would leave a problem at an initial stage without enquiring on what assumption the force of the argument rests, namely symmetry of the situation about the centre. Such a second stage might well be important in further understanding of gravitation. Lastly, of course, the argument would be a game if the earth were not round or the Australians fell off.

But even though the example is only a small step in a fuller study, here we have such a study too, while in most cases in these books this is not the case. So one is not in a position to see whether the shock is salutary or paralysing. As an example of the latter we shall consider below (just before the *Summary* at the end) a case in the foundations of mathematics. Here I shall use a simpler imaginary[1] example for illustration.

Suppose in some elementary experiments on water and rust one had found that both of them gave off oxygen, and so concluded that they must contain oxygen. Would Wittgenstein not have made fun of it saying that there were no bubbles in the water, etc.? Nobody had thought of bubbles, and hearing it at the very beginning of research in the subject, one might well be shocked into believing that one had overlooked something.

This shows two things. First, when the subject is developed and more is known, the puzzle takes care of itself. One might even agree that ' contain ' is not quite the right word. But, second, at the early stage the shock would be merely confusing; since many of the philosophical problems are at this stage, one must not be too impressed by unexpected situations.

In view of this, one no longer accepts the shock as proof that ' something is radically wrong ', but wants to know what ' building ' is to be put up. For instance, there is (or, at least, was) a certain shock in the examples which show that, often, when we say ' I mean X ' no particular mental act of meaning is involved, or that sometimes a substantive does not denote a material object or a sensation. We have overlooked something; but was it serious to overlook it as in our first example or trivial (i.e. right not to bother about it) as in our second? Wittgenstein does not tell us for which further study this clarification is to make room.

I shall consider the obvious reductionist theses suggested by these examples.

The reader is warned that some illustrations are taken from the philosophy of mathematics. This is regrettable[2] because it is probably not the potentially most fruitful field of philosophy at the present stage. It is unavoidable, not only because I happen to know a little better what is done in this field, but also because objectively the most substantial work is done in it at present.

Traditional points of View. I shall try to make out that quite natural developments of Wittgenstein's considerations may be formulated as a *reduction to the concrete;* for want of a better term I shall call it semi-behaviourism (with respect to mental acts) or semi-nominalism (with respect to abstract objects).[3] This attempt seems quite perverse because Wittgenstein is explicitly and categorically opposed to any reductionism, and contrasts the latter with a philosophy that is ' purely descriptive ' (p. 18), describing, as he says repeatedly, the grammatical[1] rôle of a word. This will not do. For, in so far as this merely relieves him of the job of defending a reduction, it is a subterfuge, at any rate as long as we mean, as we do, reduction with respect to 'communication[2] by means of language' and not with respect to an ontological position. Also, the terms in which he describes to his satisfaction the *rôle* or *use* of a word are patently of a limited kind (described above as 'concrete'), and thus there is a reduction. Finally, is it not plain that, apart[3] from their ontological views, reductionists thought that their favoured reductions constitute a clarification?

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[1] The example seems to me apt, although there is at least one passage to contradict this impression. On p. 35 Wittgenstein is quite uncritical of the question, ' What are the ultimate constituents of matter? ' However, the case below does not depend on this since we do have the example from the philosophy of mathematics just mentioned.

[2] Except in the section on Wittgenstein's theoretical position, where it is apt.

[3] An elegant summary of the outlook is given in Bernays' interesting and enjoyable review of the *Remarks on the Foundations of Mathematics*, in *Ratio* 1959. A somewhat related summary is buried on p. 136 (1.2*d*), p. 142 (2.3*a*), p. 144 (3.3) of *RFM*.

We note in passing that Wittgenstein, who is very sensitive to the misconceptions that (may) accompany traditional philosophical problems, is often peculiarly insensitive to what the traditional aims were.

With this out of the way, recall that crude behaviourism seeks to identify mental acts with particular physiological processes and larynx movements. Crude nominalism, for example with respect to numbers, identifies them with number signs. Wittgenstein's outlook constitutes a modification in at least two respects. First, he speaks freely of certain mental acts such as sensations in the usual sense, but also of seeing a *Gestalt;* [4] and of configurations with such and such a number[5] (not only of number signs), rules, etc. Second, when he comes to such mental acts as the acts of meaning, wishing, copying, etc. which he wishes to distinguish from sensations and images, he does not try to identify them with the others, but to describe their 'rôle in our language', and this is then done in concrete terms. Similarly, he does not identify numbers with number signs, but, again, wishes to describe what we 'do' with them, and this too is done in the terms just outlined. It seems to me, whatever the limitations considered below, he describes in these terms, often surprisingly successfully, situations[6] which are normally considered to involve just those mental acts and abstract objects which he eliminates. By 'successful' I simply mean that we agree spontaneously that his description expresses what we want to say. It is important to note that (justified) doubts about the exact meaning of 'concrete' do not spoil the reduction since it is sufficient if only the terms employed in the particular cases are recognised as concrete.

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[1] We shall return to the business of grammar below.

[2] The phrase occurs on p. 89. In these books, Wittgenstein is not much concerned (explicitly) with ontological questions.

[3] Of course, if one does hold a restricted ontological position, clarification will be a secondary aim, since in the first place one wishes to get rid of terms which were wrongly supposed to refer to something.

[4] More generally, seeing (hearing, feeling . . .) X as Y. See the *Remark* on p. 243.

[5] Physical realisations of an abstract object, cf. Bernays' notion of *Bezogene Existenz* in 'Mathematische Existenz und Widerspruchsfreiheit', *Études de philosophie des sciences,* Neuchâtel, 1950, 11-25.

[6] Many of them, particularly in the Blue Book. It is beyond the scope of this review to examine them one by one.

We mention in passing that detailed investigations[1] in the foundations of mathematics have revealed a similar situation with respect to a nominalist (finitist, or, more generally, predicative) elimination of such abstract objects as the totality of natural numbers or of functions. It is understood that this is applied to *certain contexts* only, usually specified by means of formal systems. Thus many of the usual proofs in arithmetic apply the principles of classical logic to statements containing quantifiers over the natural numbers, which makes immediate sense only on the assumption of such an entity as the totality of all numbers. But, for a wide class of proofs, the so-called finitist[2] interpretations which have developed from Herbrand's famous theorem, provide an elimination in a quite precise and natural sense. Of course, just as in Wittgenstein's description of the 'rôle' of an abstract term, one does not make a crude identification between, for example, a finitist and classical existential operator. Similarly, in a large part of analysis, quantification over all real numbers can be eliminated, and by use of the theory of hyperarithmetic operations of Kleene and Mostowski, the predicative[3] content of assertions involving such quantification is made explicit.

In conclusion, about what may fittingly be called Wittgenstein's practice of philosophy (in large parts of the Blue Book and small parts of the Brown) we may say this. Both his examples and the studies in the foundations of mathematics show clearly that *we have a general tendency to describe language* and, in particular, mathematical practice, *by means of concepts whose level of abstraction is higher than the minimum actually*[4] *needed.* As a corollary we have this: whatever the significant problems concerning the relation between the abstract and the concrete may be, they do not arise in the cases here considered. (More precisely, they do not arise vividly; for, of course, a successful elimination of certain occurrences of abstract terms no more excludes all need for them than for example the successful application of continuous mechanics excludes the existence of atoms; even in such cases one can ask for the relation between macroscopic and atomic laws. But this problem is less vivid than where the macroscopic laws go wrong.)

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[1] My own in this direction have certainly been influenced by the view of Wittgenstein's work here described.

[2] A variant to Hilbert's theory of the foundations of arithmetic, this *Journal*, 1953, 4, 107-129. This is a more thorough reduction to the concrete than might be supposed from the use of formal systems with arbitrarily long formulae, etc. For, in the interpretation of theorems that actually occur in ordinary mathematical practice, rather simple primitive recursive functionals are used, and those defined by means of transfinite induction with respect to well orderings near ϵ_0 are not needed. Similarly, in the next footnote, only arithmetical operations low down in the hyperarithmetic hierarchy are used (if the theorem permits a predicative interpretation at all).

[3] Kleene and Mostowski, 'La prédicativité', *Bulletin de la Société Mathématique de France*, 1960, 88.

[4] Here again: within the limited context. For, *given* the abstract conception the semi-nominalist description does *not* render the full content of the statement containing abstract terms, nor conversely. An example of the latter is the harmless statement: for all natural numbers a and b, $a + b = b + a$. For, the meaning of a quantifier is different on the finitist and the extensional interpretation, the former implying that we have a proof of the statement. However, in the particular case, we may have to agree that either interpretation is consistent with our intuitive meaning on some given occasion.

In connection with this conclusion we note for use in the next section a peculiar fact about the practice of mathematicians: they hardly ever use even that level of abstraction which is needed to give the ' interpretations ' mentioned above. Thus, though they formulate the general principle of induction in number theory, which would allow them to be non-constructive, practically all number theory is constructive. In analysis, they are non-constructive, might be impredicative because the principle of the least upper bound is so, but practically never are. In set theory they are, but do not use axioms of universes efficiently. Their real problem is not at all to get rid of the abstractions but to learn to use them.

Remark. I think this section of the present review does justice at least to the earlier portions of the Blue Book, but it may be too logically biased and even altogether too pragmatic. A remark in Bernays' review (see p. 240, n. 3) suggests a completely different point of view for discussing the later portions, namely a phenomenological (as against a logical) bias. In old fashioned language, we may look at these books, particularly the *Brown Book*, as a contribution to the study of *what is concrete*, of *what is* (*immediately*) given. Further, we should expect these books not so much to give us a theoretical understanding, but rather to make us aware of what is given. Wittgenstein's writing is so vague that it is unprofitable to look for contradictions, but there is certainly a world of difference in what he thinks[1] of introspection on page 18 (in connection with the theoretical question of the existence of sense-data) and on page 164 (in connection with seeing X as Y). Without going into a discussion of the latter phenomenon, so much at least is clear: if one is out to ' chart the concrete ' and notices this phenomenon for the first time, one will be as excited as an astronomer who finds a new planet. Now, it seems to me clear that such phenomenological studies are of interest in themselves and basic to other approaches in philosophy. For instance, in the reduction described above, the very choice of the primitive notions is, implicitly or explicitly, determined by an interest in what is concrete. Also, for example, the attributes of certainty and clarity, sometimes associated with propositions about sense-data or combinatorial facts (in contrast to material or abstract objects), should surely be analysed here with respect to ' quantity ' of concrete content; for, certainty in the sense of improbability of error, or clarity in the sense of exactness cut across the two kinds of propositions described above. But, I believe, though even in his later book *Philosophical Investigations* these phenomenological studies have not gone far enough to establish a discipline, the later work is incomparably better in this respect than the books under review. So it seems unprofitable to discuss this any further here, even if I felt competent to do so.

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[1] In fairness one should add: the actual irrelevance of introspection to the issue on p. 18, and is central rôle for the problems of p. 164.

Wittgenstein's theoretical positions. Here I do not mean the little homilies on the aims and purposes of philosophy mentioned in the introduction above, but (i) negative assertions on what cannot be said (or: is not), such as what is common or essential to those cases which he describes as families[1] of concepts, (ii) assertions on what should be accepted as a decisive criterion for (equality or difference in) meaning, such as the actual use of a term, (iii) the identification of metaphysical distinctions with grammatical ones. I call these ' theoretical ' because they constitute a basis or at least directives for a ' descriptive philosophy ' (e.g. what it should not attempt to describe). They certainly are not summaries of investigations, but much more pronouncements on their significance; for instance, in case (i) when he does not say what is common to all instances of a general term, the significance of what he does do (by way of examples) is of course greater if there is ' nothing left to be said '.

To analyse what is common to all instances of a general term in informal use, is one of the traditional[2] problems of philosophy. In its weaker form, it may be construed as requiring necessary conditions or ' what attributes apply to . . . ', in a stronger form as requiring necessary and sufficient conditions or ' which (well defined) attribute applies to . . . '. Wittgenstein objects (*a*) generally, to the introduction of an (abstract) object common to all instances of a general term, (*b*) to the assumption that a general term always corresponds to a (single?—presumably: well defined) property. He says (p. 19) that (*a*) has led to no results. Before going into the discussion it is well to remember the results of the last section, namely the tendency to introduce a level[3] of abstraction higher than is actually needed. Thus when one looks for examples, the first ones that come to mind will not use the level in an essential way. Another point to remember is that (*a*) is evidently trivial if one starts with the doctrine that all meaningful formulations must be expressible in concrete terms (and, of course, highly plausible if one holds this doctrine without saying so). So, in particular, in many cases of (*a*), especially if the general term is applied to concrete situations, he is completely in accord with (what I conceive to be) the Russellian tradition: for example by identifying *The Blue* with the class of blue objects[1] one ensures, by the no-class-principle, that the abstract term is eliminable from all meaningful contexts; in other words, if one absolutely wants to, one can have it, but it is patently not necessary. In the case of *The Blue* this is probably so except perhaps in some theoretical statements in (the) phenomenology (of colour). As to (*b*), in many[2] cases everybody would agree anyway.

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[1] The metaphor of ' family likeness ' suggests to me almost the opposite to what is intended. We analyse family likeness in terms of *genes* which, at any given time, are supposed to have a definite effect, and account for variations over a short period by the environmental changes (thermal agitation, etc.) and over a long period by mutations.

[2] Wittgenstein speaks more about the question of ' what is common to . . .' in the earlier portions, presumably because later he became engrossed in his own problems.

[3] In the last section, the levels considered were mentioned explicitly (constructive, predicative, . . .), etc. In the general case it is useful to employ a correct, but very crude measure, namely the highest type of object, in the sense of the simple theory of type, employed in the course of the argument considered.

The real issue, I believe, is this. In case (*a*), when we ask for what is common to certain formal properties or relations, for instance what abstract structure is common, for example to rotations in the plane and multiplication of complex numbers. In case (*b*) when it is evident that the property could not conceivably be expressed in concrete terms, but may be formulated in abstract terms; for example this would apply to ' mechanical procedures ' (which Wittgenstein calls a family) where one uses the non-constructive existential quantifier in the definition of recursiveness.[3] More explicitly, the issue in (*a*) can be put as follows: ' Why this mouthful about " abstract structures "?[4] What one shows is that formulae of a certain shape, instances of a formula schema, are valid in both cases (so-called group laws) and then one formally derives consequences from this. Of course, the notion of validity is used, but that, after all, can be expressed by: these formulae are formal consequences of axioms for rotations and complex numbers respectively.' Or, a little less (more?) brashly: ' Well, there *is* a conceptual distinction, but it's a distinction without a difference.' I think we are back to note 4, page 242. The distinction is simply not vivid unless there is a patent difference; but, somewhat reluctantly, I am persuaded that it is real, by rather recent developments in mathematics where set theoretical operations are applied in a highly impredicative manner: they seem evident only on the conception of an abstract structure.[5] The question is of course not settled if one regards this latter evidence as problematic. I believe the situation is not at all clear, but, at least, here we have an area where the issue is vivid. As to (*b*) it is unavoidable to refer to detailed discussions: for example, concerning mechanical procedure, the discussion by Turing[1] or a more recent (and far less satisfactory) note[2] concerning finitist proof. It seems very natural that one is not instantaneously convinced of correct characterisations even if the arguments are good on reflection. After all, we do not have much experience with these matters, and it would be amazing if our intuitions (first thoughts) were to be both reliable and confident: the degree of conviction carried by the same experiment in the natural sciences or the same argument in mathematics certainly changes with the development of the subject. (This is, of course, a corollary to the familiar assertion that standards of rigour are currently established in the course of developing a subject and not before.)[3]

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[1] Images would be regarded as objects here on the outlook of the last section.

[2] Cf. Bernays' remark on *The Good*, loc. cit., footnote 10.

[3] I use here the ' family of concepts ' of mechanical procedures because Turing's argument seems to me rather stronger for this case than for (humanly) effective procedures.

[4] The strong opposition of some mathematicians to the Bourbaki conception of mathematics (and Bourbaki's self doubts in the introduction to their volume on set theory) could be phrased similarly. It is to be noted that its aims are just about opposite to those described in the previous section which lead to reductions.

[5] Note that these cases are not ' too general ', because the really general conceptions treated in philosophy such as proposition or property are definitely of a higher level of abstraction still. Thus to formulate the general notion of a well-defined property P (= for every object b either P applies to b or P does not apply to b) we need a non-classical interpretation of the logical constants which is meaningful when applied to expressions which are not necessarily either true or false, since otherwise the condition in brackets would be empty. For a coherent and, in my opinion, very promising approach see the work on type-free logic by Ackermann and Schütte, for example, *Math. Zeitschrift*, 1954, 61, 160–179.

The popular response to the question of what is essential to a concept (again, primarily in the sense of 'necessary' and not of 'necessary and sufficient') is, of course: for what? This rather harmless piece of philosophy would naturally be followed by investigating whether there is something that is essential for all its applications or the huge majority of them. Thus we should certainly assert that transitivity (if $a = b$ and $b = c$ then $a = c$) is essential to the concept of equality. We should regard deviations[4] from this in actual uses of equality (e.g. visual comparisons) as inessential or as incorrect, depending on further reflection on the cases involved; certainly, unless one starts with the doctrine that all details of actual usage are decisive, there is no inconsistency in this. Returning to note 5 on page 245, present day mathematics has certainly gained a great deal in clarity by the rather surprising discovery that relatively few abstract structures were essential to the proofs in the greater part of current mathematics. (Here, the emphasis is not on the abstract structure as an object.[5]) In any case, in so far as 'putting things in order' (pp. 44-45)[1] is concerned, it is surely as helpful to find a precise concept from which there are few deviations as to point out differences, and differences between differences, etc.

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[1] A. M. Turing, 'On Computable Numbers, with a note on the Entscheidungsproblem', *Proc. London Math. Soc.*, 1936, **42**.

[2] *Proc. Int. Congress of Math. Edinburgh*, 1958, 289–299. This should be read in conjunction with the clear philosophical arguments in Gödel's contribution to the Bernays Festschrift, *Dialectica*, 1958.

[3] Incidentally, the same applies to the delineation of the subject matter itself. Hence the futility (at the present time) of pressing too hard the question: 'What is philosophy?'

[4] On p. 125 Wittgenstein compares the search for essentials of a (different) term with trying to find the real artichoke by stripping it of its leaves. I am sure he confused artichokes with onions. But what he actually says, fits our case much better. There may be some indefiniteness about the extent of the core of an artichoke, but the distinction between core and leaves is real (and essential).

[5] We may note that a proof of the validity of a theorem in such and such an abstract space is often important not because of the extended applicability of the theorem but because it is an answer to the question: What is essential to the theorem?

As far as *actual use* of words is concerned, I am in the unfortunate position (in view of the last paragraph) that I have to begin with a distinction. 'Actual use' may refer to the words spoken: this has the attraction that here we have a subject matter for philosophy comparable to the 'hard' experimental facts of the natural sciences or the combinatorial facts of mathematics to which one can refer when the theoretical framework creaks. Furthermore, since it includes everything that is said, it would seem to leave room for all things between Heaven and Earth. But 'actual use' may also mean the *real* rôle of the word (as Wittgenstein puts it), undistorted by the vagaries of linguistic expression from which philosophy should free us: this has the attraction that now we are getting real knowledge. But, unfortunately, the latter is, in general, achieved at the cost of the former. Now, I believe that in the cases of elimination of abstract terms described as successful in the last section, there seems no doubt about the actual use (in the second sense), just as one does not doubt that, in the future, the very simple observations of chairs and tables will continue to be taken as 'facts'. But in other cases the whole problem is thrown back to what is conceived as the real rôle (cf. *RFM*, p. 152, 8.3*b*).

On page 137 of *RFM* (1.3*a*) I also questioned the value of the 'reduction' of metaphysics to grammar. Of course, the attraction here is again (to use his own expression) that the occult is eliminated and replaced by ordinary everyday words. This was, indeed, also the attraction of the early syntactic approach to logic, where the syntactic rules (for operating with the formal expressions) were of an elementary character. But then, I believe with Carnap, the rot set in: simply because the objects talked about were symbols, for example, the notion of a set of formulae true under such and such an interpretation, was called 'syntactic'.[2] This notion is no less problematic than the notion of 'truth under the given interpretation'. The reduction to grammar seems to suffer from a similar defect: I see no evidence that the grammatical distinctions which are to replace (problematic) metaphysical ones, are going to be described by means of less problematic concepts. More specifically the reference to grammar is deceptive for at least two additional reasons: First, perhaps for historical accidents, one does not usually consider such questions as 'what is a noun' in a theoretical way; the question is undoubtedly grammatical, and hardly more tractable than 'what is an object'. Second, while it seems to me perfectly apt to speak, as Wittgenstein does, of the grammatical rôle of a word in a language, the difficulty of formulating this seems to be of an entirely different order from school grammar where one classifies words into categories often even independently of their position in a sentence. Without wishing to press the analogy, one is reminded of the difference in the conceptual problems involved in simply weighing things and in formulating the relation between mass and weight in terms of the distribution of masses in the universe. In short, grammar could be very difficult.

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[1] It is revealing (top of p. 45) that Wittgenstein's stated aim is to *separate* the books. The metaphor could be developed: if the room (= powers of memorising) were very large, one could just put every book by itself, and contemplate it in isolation.

[2] There are certain theorems in algebra which are fully formulated only if one considers such sets of formulae. This is of course no reason for calling these sets 'syntactic(ally defined)'.

However, it is to be emphasised that in so far as the 'cult' of actual usage or of grammar is objectionable this is not so because these notions are imprecise in places or present problems of their own. But rather because such problems seem to arise just at the very juncture where the 'metaphysical' concepts which they are to replace, present serious problems.

Remark. The present section, which is almost entirely critical, may convey the impression that no theoretical positions of any generality, in particular no impossibility or undefinability assertions, can be established. On the contrary, I believe that some of the *most outstanding results are such negative results, only Wittgenstein's approach is particularly unsuited to such work.* In particular, their significance is connected with the fact that Wittgenstein's general theoretical positions are wrong:[1] Suppose the impossibility of a characterisation by certain means (e.g. mechanical procedures) is to be shown, where the means considered form a family of concepts in Wittgenstein's sense. Suppose further we find an abstractly formulated property (here: recursiveness) which is certainly satisfied by all members of the family, and possibly by things outside it. *If we now establish the impossibility of achieving the required end by all methods which have the abstractly defined property, then we have a negative result which is unaffected by uncertainties* about the exact extent of the family of concepts considered. (It is of course not even required that the 'family' should have an exact extent.) I believe the epoch-making character of the work initiated by Gödel rests largely on satisfying all these suppositions (for details, see *RFM*, p. 154). The possible need for an abstractly defined property is also apparent here; namely if all the well-defined properties which are common to such a family (and sufficient to derive the required conclusion) are definable only on a higher level of abstraction. Now, if one rejects the use of abstract concepts as a means of philosophic analysis (or, at least, considers an explanation in concrete terms more fruitful) one will tend to reject the particular interpretation just discussed. Since this interpretation is certainly natural it seems understandable why Wittgenstein objects so strongly to attributing philosophic significance to the impossibility results of mathematical logic.

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[1] In *RFM*, p. 153, I stressed the wildness of Wittgenstein's arguments in connection with Gödel's work, but left open (line 8) the source of this. The analysis below may be a partial answer.

It may be worth while to add at this point a comment on the use of mathematical logic in philosophy.[1] It is certainly true that some ambiguities with ' and ' and ' or ', for example, the negation of: *A* or *B*, and ' all ' and ' some ' are resolved by the current logical notation. But the case for this seems hardly different from teaching Latin as an aid to good English: a clean intellectual home (*gute geistige Kinderstube*) can look after both. On the contrary, emphasis on this point detracts attention from the places where mathematical logic appears *essential*, either *for deriving results as above or even for formulating*[2] *a philosophical, for example an ontological conception*, sufficiently sharply to draw conclusions. The latter is not surprising if one remembers that often a physicist's conception can be formulated only by the use of mathematical machinery not known to him, for example Maxwell's theory for Faraday's conception. Just as in physics out of the way matters are sometimes more tractable[3] than what is familiar, open to inspection and experimentation, so, here, we have to take as an example two ontological positions in ' higher ' mathematics, namely these: (*a*) the natural numbers are abstract objects which exist independently of us, (*b*) we create them. Wittgenstein objected to (*a*), essentially because (*a*) had ludicrous associations for him, and probably would have found something wrong with the formulation of (*b*). (These are the philosophical positions referred to on p. 241.) Without any mathematics, one can make the contrast between (*a*) and (*b*) vivid by reference to the irreverent conundrum: Can God create a stone so heavy that he does not know how to move it? Now, if the totality of possible stones and blueprints for moving them were fixed, this question would have a definite answer in either of the two natural (though different) senses: (i) does there exist a stone that defeats all blueprints or (ii) does there exist a blueprint that defeats all stones. However, if these totalities grow the answer would, in general, change from time to time (even for a given sense) according to whether stones or blueprints are getting the upper hand. Finally, there is the situation in between where an unreflective creator assumes in ignorance the second possibility to hold, while a shrewd guess about his methods shows that he has reached a steady state (with respect to the present issue).. In consequence, a ' definition ' of an object *a* by the condition: *a* is the oldest unmovable stone if there is one and the oldest stone if there is no unmovable stone, certainly singles out a unique stone if the totality is fixed, does not in the second case, and may do so in the third. This already brings us fairly near a mathematical formulation, namely this: If (*a*) holds then, whatever else (*a*) may mean, quantification over the totality of all numbers has a well defined sense[1] and classical logic may be applied to statements so formed. On (*b*) this is certainly not justified, and on the strict interpretation, namely the second and not the third possibility above, it would not make sense to use quantifiers. But, if one asks, whether a given statement essentially presupposes (*a*), in the sense that it could not be proved by means of principles which are evident on (*b*), then it is clear that a mathematical characterisation is needed for an answer. If (*b*) is interpreted in the strict sense, we get a rough illustration of what is meant by a finitist proof. It is to be observed that one is not giving an *arbitrary* technical meaning to the rough conceptions (*a*) and (*b*). There is certainly no virtue in using mathematics in order ' to be precise ' at all costs. The unmotivated axiom-mongerer in philosophy is on a par with the applied mathematician who ' sticks in another parameter ' to fit his curve.[2]

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[1] This is a strictly personal opinion, shared perhaps by some logicians with a mathematical bias but for example not by the majority of even those professional philosophers who strongly support mathematical logic in a philosophical education.

[2] This is certainly denied by Wittgenstein, and his position, in turn is criticised in *RFM*, p. 143 (3.2). However, mathematicians, too, shy away from the responsibility of having something to contribute to ontology (or physics, for that matter), cf. Fränkel Bar-Hillel, *Abstract Set Theory*, North Holland Press, Amsterdam, p. 344.

[3] Thus in the seventeenth century one could say informatively why the moon has such and such a period (given its distance from the earth), while one had only a futile linguistic answer to: why is glass transparent?

Summary. I think an explanation has emerged of the two striking facts noted at the beginning of the review.

(i) When dipping into the books one is offered imaginative examples pointing out some ' blind spot ', surprisingly concrete reformulations of what appears at first to presuppose higher levels of abstraction, and striking, but easily verified phenomenological facts. The latter are all the more attractive because Wittgenstein is evidently genuinely interested in them, and not only as illustrations of a general thesis. There is (I believe) no particular inherent difficulty in the points made or strangeness in his point of view. Also, at the beginning, he sets himself rather clear problems and aims. But disappointment sets in when he begins to waver, evidently goes in a new direction, *but never returns to re-examine the earlier positions.* Also, one is put off by the misconstruction and misinterpretation of evidently significant problems.

(ii) No doubt, one is inclined to have higher standards in one's special field. But, I believe, we also have the following objective fact: the concrete content of mathematical assertions is far less significant than of the situations described in the books under review, in all but the most elementary parts of mathematics such as the addition and multiplication of (small) numbers.[1]

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[1] More generally, the comprehension principle in the theory of types. This clarifies the footnote on p. 138 of *RFM.*

[2] The latter is despised by the physicists. This seems to show that they do not share completely Boyle's view that error is less damaging to progress than vagueness.

As to content, the ideas of the book seem to be most relevant to the discipline which studies what is concrete (and whose exact delineation is yet to be evolved). On the positive side there are descriptions of little noticed phenomena (phenomenology) and reductions to concrete terms of many situations that are in the first place viewed abstractly. As described above a wider sense of ' reduction ' is appropiate than is used in crude positivism or nominalism. This work shows convincingly a natural tendency of being unnecessarily abstract. On the negative side we have Wittgenstein's theoretical positions; on analysis, they are seen to be cogent consequences of philosophical doctrines which, roughly speaking, overestimate what can be done in concrete terms. Since the former seem to be easily refuted they are useful in *reductio ad absurdum* arguments applied to the latter.

As an introduction to the significant problems or traditional philosophy the books are deplorable.[2]

G. KREISEL

Correction to RFM. On page 153 (9.1) of *RFM* I asserted that Wittgenstein's remarks on the ambiguity of the notion of an enumeration of (number theoretic) functions were unfounded. This assertion was correct because he was explicitly concerned with Cantor's set theory where a function f is identified with the set of pairs $< n, f(n) >$, and the notion of enumeration is unambiguous. However, his remarks can be given a little more sense if an intensional notion of function (*rule* of calculation) is considered. If we take the familiar notion of a rule of calculation expressed by a set of (recursion) equations, at least three meanings of enumeration suggest themselves:

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[1] Wittgenstein attempts to analyse their ' concrete content ' elsewhere, loc. cit. note 2, p. 238 above. In particular, he recognises and emphasises the problem of analysing the concrete content of such simple operations as *copying*. It may well be that the approach in the books under review may help (some people) with this sort of problem. Personally I suspect that just these basic operations will require for fruitful analysis a very sophisticated conceptual apparatus (cf. the problem why is glass transparent, in note 2, p. 249 above).

[2] This is largely based on a personal reaction. I believe that early contact with Wittgenstein's outlook has hindered rather than helped me to establish a fruitful perspective on philosophy as a discipline in its own right, and not merely for example as methodology of highly developed sciences. In fairness (I do not know whether to him or to me) it should be added that most philosophy of the day including consciously anti-Wittgensteinian brands, does not seem much better. Exceptions are, in my opinions, the philosophical writings of Bernays and Gödel, some of which are quoted in the text, but also for example the latter's contribution to the Schillp volume on Russell, which is not. The usefulness of the ideas of these writers is, at least in my opinion, not confined to the area of mathematical philosophy.

(1) an enumeration of the symbolic expressions of the rules, that is, of the equations, in which distinct equations get distinct numbers; (2) one in which distinct rules which define functions with the same course of values, get the same numbers, others get different numbers; (3) one which associates numbers not with the symbolic expressions of the rules, but with (suitable finite) sets of values of the functions defined by means of such rules. Now, (1) is evidently possible, (3) is trivially impossible since the number associated with a function depends only on a finite set of its values, (2) is seen to be impossible by means of a not altogether trivial argument,[1] if we require the enumeration to be given by a rule of calculation too. And even though non-enumerability is established both in senses (2) and (3), these senses are certainly different.

Causality—The Place of the Causal Principle in Modern Science. By Mario Bunge.
Harvard University Press; London: Oxford University Press, 1959. Pp. xx + 380. 60s.

THE nature and extent of causal relations and their bearing on determinism are perennial questions in the philosophy of science, and the variety of modes of causation in science which now have to be considered has made many traditional discussions of the subject obsolete. We must therefore be grateful to Professor Bunge for this detailed survey of causality and determinism in relation to the various kinds of theory that appear in modern science, and for his criticisms in the light of these of many traditional views. Thus, he questions the commonly accepted reduction of causality to invariable succession or to predictability; the view that functional laws may in themselves express causality; the view that causality is valid at least throughout *classical* physics; and the positivist assertion that the progress of science brings unification of explanatory concepts and theories. On the last point he makes the very pertinent observation that, on the contrary, science shows progressive *differentiation* of concepts and laws—an observation which escapes us only when we become so impressed by the *formal* success of unified mathematical theories that we forget that these have to be interpreted in widely different empirical situations, and that every such interpretation brings its own conceptual complexity.

Differentiation is in fact the keynote of the book. There is, Bunge argues, not just one problem of the nature of determination in science, but many modes of determination of which causality is only one, and which also include law-like relations which he characterises as structural, statistical, teleological, and assertions of class-inclusion. He defines *determination* in terms of three necessary and sufficient conditions: the *genetic principle* (' nothing comes out of nothing or passes into nothing '), the *principle of lawfulness* (that there are regularities of events), and the *ontological interpretation* of the laws involved. Causality itself is that tighter form of determination which implies a one-one relation between two events C and E such that ' If C happens, then (and only then) E is always produced by it.'

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[1] Partial recursive functionals and effective operations, *Constructivity in Mathematics*, North Holland Press, Amsterdam, 1959.

The distinctions here made and their applications are illuminating and are worked out with copious references to the science and philosophy of the past and present. Many points of detail deserve attention, but I shall confine my remarks to what is likely to be the most controversial feature of the book, namely the ontological interpretation of determination, and its reflection in the notion of *productivity* in the definition of causality quoted above.

Bunge wishes to insist, against all the heirs of Hume, that there are laws which ' work objectively . . . are immanent in things, . . . are modes of being and changing of things . . . are discoverable and not inventable ' (p. 250). This ontological assumption is so closely woven into Bunge's account that it is difficult to discover whether he is intending to provide specific arguments to support it. Apart from a rather sketchy quotation of an inconclusive refutation of Hume, his arguments continually *presuppose* the ontological nature of causality and determination in general, and use the presupposition to refute or to help in refuting various positivist accounts of causality. Thus the reader is left in a difficulty, for although this reviewer at least is initially sympathetic to an ontological as opposed to a positivist interpretation, it is not clear how the distinction between them can be upheld unless it is shown that it makes *some* difference to be an ontologist—not of course a directly empirical difference (no one expects so metaphysical a position to be in a crude sense empirically testable) but at least some difference that is more than a comfortable feeling of ' being in touch with reality '. The difficulty here is that Bunge seems to operate with an almost closed circle of concepts, few of which admit of any interpretation which meets the anti-ontologist objections. Thus a typical argument is the following: a necessary condition for a causal relation is said to be the *production* of the effect by the cause; only *events* and not qualities or dispositions can be productive; *states* of a system are qualities and not events; hence mere regular successions of states are not causal. But when we try to break into the circle by enquiring how we can recognise *productivity* or *event*, Bunge becomes elusive—I cannot find anywhere in the book an independent discussion of what is meant by either term.

Perhaps some light can be thrown on the positive content of Bunge's ontologism by examining the views he uses it to refute, together with the other reasons he gives for refuting these same views, for if certain kinds of scientific relation are distinguished from others on grounds which clearly do ' make a difference ', and if these are then said to be ' ontological ' and the others not, this in itself may be made to give a sense to ' ontological '. Several of Bunge's points might be relevant here:

(1) He argues that causation cannot be reduced to predictability, since (*a*) prediction is never exact, whereas he has defined the cause-effect relation as one-one; and (*b*) ability to predict, for instance the explosion of an atomic bomb, does not entail that we have a complete causal explanation of what happens.

(2) He argues that causality is not reducible to mathematical forms of relation, since these are not directly testable, and the same form may be interpreted in many different ways, some causal and some non-causal.

(3) He argues that causal explanations in advanced science demand enquiry into the intelligible mechanism of change. Mere succession or mere logical form does not lead the scientist to feel he has understood the change, as is clear from the way in which theoretical explanations are *summarised*—the detailed logical framework is omitted and the ' stuff' of the explanation is described, showing that it is the latter which is thought to be the essence of the explanation.

Points (1*a*) and (1*b*) are acceptable enough, but it is doubtful if the appeal to ontology can be made to rest on them. All that is shown is that the causal relation as Bunge has defined it is not universally applicable in science, and this he himself argues later in the book, where he points out examples of types of determination which are not causal. The arguments against reduction to predictability do not show that other forms of determination are not so reducible, and indeed it may be that just because of arguments such as these, it is other forms of determination that are found in science, and not causality as Bunge has defined it.

The arguments (2) and (3) seem to offer firmer ground for ontology, but unfortunately are not pursued in any detail. This is a pity, because it is more than ever apparent after reading all that is said about ontology and natural science in this book, that the most hopeful way to approach the question is by consideration of what is involved in the ' interpretation ' of a mathematical theory, what it is for an explanation to be ' plausible ' or ' intelligible ', and how far such characteristics are implied in the way laws and theories are used. If it is found that the inferences we wish to make from laws and theories presuppose a distinction between law-like connections and merely accidental connections, then this distinction itself may provide good reason for describing the one and not the other as ' ontological '. Two investigations along these lines would seen to be relevant: one is the question of counterfactual conditionals, which Bunge curiously does not mention; the other is the question of the reality-status of models for scientific theories. In the first case, the fact that we feel entitled to make counterfactual inferences from law-like connections but not from accidental ones gives some grounds for ascribing ' ontological necessity ' to the former; and in the second case, to show that we make inferences from theoretical models in the same kinds of way that we make inferences from situations involving objects whose reality is unquestioned, gives some grounds for using the predicate ' ontological ' of explanations involving such models.

However, this is not the book Professor Bunge has written, and it may seem churlish to concentrate this review upon the suggestion that he should have written a different one. But it is impossible not to regret that so detailed and informative an account of the workings of causality in modern science has not been used to enquire into what a post-positivist philosophy of science must regard as the central problem.

MARY B. HESSE

Experience and Reflection.

By E. A. Singer, Jr. Edited by C. West Churchman.
University of Pennsylvania Press, Philadelphia; Oxford University Press, London, 1960. Pp. xv + 413. 40s.

THIS book is the result of Professor Churchman's editing of a manuscript left uncompleted at Singer's death. Churchman contributes a short introduction in which he gives a summary of Singer's thesis and indicates briefly the respects in which the work is incomplete.

It is in three sections. The first part contains a discussion of traditional theories of knowledge under three main headings. According to Singer, Rationalism holds that ' No knowledge of law implies knowledge of fact; all knowledge of fact implies knowledge of law ', Empiricism holds that ' All knowledge of law implies knowledge of fact; some knowledge of fact does not imply knowledge of law ' and Criticism, in the Kantian sense, holds that ' Some knowledge of law implies, and some does not imply, knowledge of fact; some knowledge of fact implies, and some does not imply, knowledge of law '. But there is a fourth possibility, namely that knowledge of law depends on knowledge of fact *and conversely*. One aim of the book is to examine this possibility in relation to physics and biology. This purpose, although implicit throughout, seems sometimes to be lost sight of, no doubt because of the incompleteness of the manuscript.

The second part consists mainly of a consideration of the nature of scientific questions and answers in relation to physics and contains discussions of 'the given' and of the mutual adjustment of data and 'formal images', that is, models, which are involved in theories. It is in the light of his account of scientific questions and answers that Singer takes scientific procedures to show that knowledge of law depends on knowledge of fact, *and conversely*.

The third part concerns the 'biocentric' sciences and proceeds, in the belief that the properties of living things cannot be entirely subsumed under the laws of physics, to discuss the construction of biological concepts which are independent of physical imagery but which never conflict with it. For this purpose, Singer attempts to define 'living body' not in terms of structure, nor entirely in terms of nutritive and reproductive functions, but in terms of heredities. Toward the end of this section there are obvious signs of incompleteness and some important questions are left insufficiently discussed.

Since Singer's account of scientific questions and answers is fundamental to his main thesis I shall confine my comments to this. Science is faced with questions of fact but the conclusions of science are not answers in the form of statements of fact; they are in the imperative mood and not the indicative. This, Singer argues, is shown especially clearly by a consideration of metrical questions. For example, a surveyor asked for the size of an angle would, if he gave an *answer*, say 'The angle in question is an angle of m°', but what he in fact says is 'The angle measured is to be taken as an angle lying within the range $(m \pm p)^\circ$'. This, says Singer, is not an answer to the question but a *response* to it. There are three points of difference. The answer would be about 'the angle in question', the response is about 'the angle measured'; the answer would have 'is' as coupling verb, the response has 'is to be taken as'; the answer would give a specific value, the response gives a range. Thus the response is not an answer to the question asked and it is a command rather than a statement of fact. An answer-dictating response would be obtained if $p = 0$ in the expression $(m \pm p)^\circ$ and this is a limiting conception which the scientist can progressively approach but never reach. Science is therefore faced with many questions which are unanswerable but nevertheless meaningful because we know what would count as answers. This account naturally necessitates a rather special account of fact, law, and the relations between them.

Whatever is to be said for Singer's main thesis, he has surely been misled by inessential features of scientific discourse and by his failure to give due weight to the contexts in which questions are asked. The scientist who asks for the size of an angle knows that the best he can expect is a range of values, just as the pure mathematician knows that for some of his questions he can expect a specific value but for others only a range. Otherwise, we suppose them to be ignorant of the power and limits of the methods at their disposal. It is surely more plausible to regard the scientist as asking for a range of values, even when the words of his question do not indicate this explicitly, than to suppose him to be asking an unanswerable question. This is what he means and what other scientists take him to mean, even if the form of his question does not convey this to the layman. Moreover, if we insist on the imperative mood in the response, it is more plausible to regard the scientist as asking what he must take to be the value he requires rather than to suppose him to be so easily fobbed off with a command when he wanted a statement of fact.

But need we insist on the imperative mood in the response? What warrant have we for choosing between the responses ' The angle measured is to be taken as an angle lying within the range $(m \pm p)^\circ$ ' and ' The angle in question lies within the range $(m \pm p)^\circ$ '? The behaviour of the scientist in accepting answers to his questions perhaps gives us some reason to suppose that he understands them to be properly stated in the second form. Scientists sometimes do take their conclusions to be answers to their questions of fact and if our account fails to allow this we fail to understand what *they* mean by ' questions ' and ' facts '.

P. ALEXANDER

Greek Culture and the Ego. By Adrian Stokes.
Tavistock Publications Ltd., 1958. Pp. 101. 15s.

EUROPEAN civilisation was formed essentially during the Golden Age of Greece. Science was born then; but art as well was given its Western mould at that time. It is of great interest, therefore, to investigate the forces at work in the classical period. By interpreting Greek art and finding out how it came about and what it means, we may also gain insight into science.

Adrian Stokes has taken the psycho-analytic approach to explain Greek culture, following Freud and Mrs Klein. Freud himself had applied his theory to such problems, though his concern was mainly with primitive cultures. H. Kelsen (1946) was the first to turn his attention to the Greeks with the purpose of elucidating scientific conceptions; his viewpoint, however, was that of the sociologist. Professor Dodds, in his *The Greeks and the Irrational* (1951), showed how much infantile phantasy remained alive underneath the clear rationality of the Greeks. Recently (1957), in a lecture on ' The Origins of Science ', I tried to show that the development of Ionian philosophy, from Thales' element of water to the atom of Demokritos, is similar to the way in which the human infant gains knowledge about the real world outside of him.

Mr Stokes begins with discussing a key concept of the Greeks—

καλὸς κἀγαθός—the idea that whatever is beautiful is also morally good. Greek art helps us to understand this statement which is often regarded as either obscure or trite. The Greek nude, for example, still appears to us not only as beautiful, but also as a realistic representation of the female body. If we look at the art immediately before the classical age, say, of Mycenae or of Egypt, we are struck by the fact how life-like Greek art is in comparison to its predecessors. The simple and clear outline of a Greek temple is another example showing that the classical mind was relatively free of the distorting phantasies which rule the more primitive 'barbarians'. This rationality was achieved by a great effort of repression which, as Freud explained, is always needed in order to build a civilisation. We have to renounce the immediate satisfaction of primitive desires and learn to sublimate them, so that energy becomes available for other purposes. Thus the infantile fears and phantasies are defeated that prevent us from seeing reality as it is.

Greek rationality is reflected in their morality. The Greeks believed in knowledge, of themselves as well as of the external world. *Γνῶθι σεαυτότ* and *'Επισιήμη ἐστὶν ἀρετή* were their slogans. They preached moderation in all things—*Μηδὲν ἄγαν*—and they abhorred mania—*ὕβρις*. They felt that a happy man is likely to be a good man, and that happiness and beauty go together: they strove for harmony (a Greek word, after all). Finally, some of the philosophers, like Xenophanes, recognised the Olympians to be a human invention, a projection of their own fears and wishes. In short, the Greeks believed in Man rather than in the gods. This attitude of balance and tolerance demonstrates the emotional maturity which they had achieved at the height of their civilisation. Or, to put it into psychoanalytic terms, instead of an authoritarian ego-ideal, the Greeks adopted what Adrian Stokes calls the ego-figure. While the ego-ideal arises from a primitive super-ego, the ego-figure stands for the integrated and adult personality. This is expressed in Greek art: the emphasis is on the whole and sound body. The tyranny of the super-ego is easily seen in the persecutory feelings that rule primitive society and can be found in early Greek myth, e.g. the Erynnies. The ego-figure arises more from the depressive anxieties that are aroused in us when we try to repair damage and bring together good and bad. 'The concept of beauty,' Stokes writes, 'projects, not the ego-ideal, but the ideal of an integrated system.' This is, once more, the balance that characterises the adult attitude.

A high level of creative achievement and of moral understanding was reached during the classical age but, alas, it did not last very long. Under the impact of military defeat and with the subsequent influx of Eastern religion, Greek civilisation crumbled; and the Greeks themselves regressed to a more primitive state. Until our day, they have not again produced either science or art.

The concept of ego-figure which Adrian Stokes proposes is, I think, very powerful and suggestive of application. Not only in art, but in science as well, integration is what we aim for; an instance is the integration of knowledge in a theory. Our ' body ' of knowledge must equally be whole and sound.

The philosophy of science cannot be divorced from the history of science. If we want to understand our basic conceptions, their power and their limitations, we have to investigate not only their logic, but also their origin. Burnet remarked once that science is ' thinking about the world in the Greek way '. This book by Adrian Stokes helps us to understand the Greeks better and, thus, also science.

ERNEST H. HUTTEN

Darwin's Place in History. By C. D. Darlington.
Basil Blackwell, Oxford, 1959. Pp. ix + 101. 9s. 6d.

THIS book, seventy-four pages long and with a bibliographical appendix of some twenty-five pages, in the words of the author, is an account of the ' issues that matter most in the work of Darwin, of their origins before his time and of their effects today '.

In particular an attempt is made to answer the following four questions :

(1) What was new in the ideas contained in the ' Origin ' ?

(2) Who was responsible for them?

(3) What effect did they have at the time?

(4) What do they mean for us now?

The author also claims that the quotations ' put together (for the first time) the basic documents of evolutionary theory ', and that the ' bibliography should enable those who wish to go further to follow the inquiry to its limits '.

Despite the author's disarming comment that his book is only a ' slight account ' of the questions asked, it must be said that he has not done justice to the essentials of the historical problems concerned with questions (1) and (2). For example, in his account of the history of the idea of evolution, after mentioning Leonardo da Vinci, Montaigne, and Descartes, he gives a reasonably full (for this book) description only of Erasmus Darwin's views on evolutionary theory.

Now the erroneous assumption underlying Darlington's account of the history of the idea of evolution is that this idea is one of those with a reasonably clear cut origin. In fact the idea of evolution rests on a number of other important notions, e.g. change, development, progress, time, etc., each of which has its own long history, and all of which have to be brought together in order that the major idea can be worked out.

This criticism that the author's view of the history of the idea of evolution is over-simple equally applies to his account of the history of the theory of natural selection, whose basic notions are not only of considerable antiquity but are derived from diverse sources other than biological ones.

The outline of the vicissitudes of the ideas used by Darwin during the first quarter of the nineteenth century is of considerable interest. In particular, the rough handling by orthodox thinkers of W. Lawrence after the publication in 1819 of his evolution-tainted *Natural History of Man* was a lesson not lost on Darwin. It undoubtedly made him very hesitant in publishing his own work, and it was not until the arrival of Wallace's essay that he was pushed into publication.

One of the most fascinating aspects of the history of Darwinism is that of Darwin's own gradual retreat from his Theory of Natural Selection to the view that the inheritance of acquired characters was the principal cause of evolution. Darlington's account of this and of Darwin's ambivalence in having to face the crucial issue of applying his theories to Man himself is done without the usual reservation found in most books on this subject.

The final chapters on the importance of Mendelian genetics or Darwinian theories and the wider application of these theories to other subjects are more convincingly sketched in than the rather thin earlier chapters.

Finally, it must be said that the bibliographical appendix is not quite as adequate as the author supposes. To take one example, although mentioned in the text, there is no reference in the bibliography to Malthus's *Essay on Population*, nor to any of his precursors, a somewhat surprising omission.

R. P. GOULD

The Principles of Science. A Treatise on Logic and Scientific Method.
By W. Stanley Jevons.
Dover Publications, New York, 1958. Pp. liii + 786. $2.98

THIS is a paper-backed reprint of the well-known work of Jevons which appeared first in 1874. The setting of the pages, which reproduces exactly the lay-out of the second edition (stereotyped in 1877), is presumably the result of a photographic process. In a new introduction Professor Ernest Nagel gives a sketch of Jevons's life and explains briefly his contributions to (i) the class calculus, (ii) the theory of ampliative inductions as an application of hypothetico-deductive method, and (iii) the theory of probability. He makes it clear, of course, that in these three fields Jevons elaborated the work of Boole, Whewell, and Laplace, and he points out that Jevons's development of what he inherited was not in all respects perfect. But he rightly stresses the point that the attempt to bring together the three notions mentioned above was an important step in the history of the philosophy of science.

Perhaps no one since Leibniz had thought of all these topics together. We now know that a formal logic without quantifiers is inadequate for the purposes of science, and we are properly sceptical about the thesis that ampliative induction can be justified by a probabilistic argument from the principle of indifference; but it is evident to us that a really comprehensive book about the principles of science would have to include all the topics Jevons discussed. On the other hand, as Professor Nagel remarks, it is rather curious that Jevons did not take note of developments in the philosophy of geometry about which he might have learnt from Clifford, and that, unlike John Stuart Mill, he did not consider the peculiar problems of the social sciences in which he was himself a distinguished practitioner.

In their advertising material the publishers draw special attention to Jevons's account of the logical machine which he exhibited to the Royal Society in 1870. It is pleasant to record that the machine is now safely housed in the Museum of the History of Science at Oxford, intact except for the catgut by which its moving parts were originally connected. If computers acquire from their creators a disposition to ancestor worship, we may some day see them queuing in Broad Street to pay their respects to this simple forefather of their race.

William Kneale

Roots of Scientific Thought. A Cultural Perspective.
Edited by P. P. Wiener and A. Noland.
Basic Books, New York, 1957. Pp. x + 677. $8.00.

This volume consists of a collection of essays written by thirty scholars. The essays range over the entire history of science, from the ancient Greeks to the contemporary cosmologists. The book is divided into four parts covering the main periods of development, i.e. the classical heritage, from rationalism to experimentalism, the scientific revolution and, finally, from the world-machine to cosmic evolution.

There is a great deal of interesting material to be found in this book, though most of it is not new and also the approach to it is conventional. It is the usual biographical treatment that many of the writers apply to their theme; they describe what the Greek philosophers or Galileo, Gilbert, Copernicus, Kepler, Bacon, etc. have said. Occasionally, a writer takes a wider, more comprehensive, view. Koyré, for example, investigates why Galileo accepted certain tenets of Platonism in his fight against the Aristotelians. Zilsel describes the genesis of the concept of scientific progress. The article by M. G. Evans on Aristotle, Newton, and the theory of continuous magnitude is, I think, illuminating; he discusses how closely this theory is connected with our intuitive notion of uniform motion. Thorndyke's essay on the seventeenth century is very erudite. Toulmin's discussion of the idea of crucial experiment as illustrated by Priestley's and Lavoisier's disagreement about the phlogiston is very relevant. Ellegard writes interestingly on the reaction of contemporary philosophers like Whewell to Darwin's theory.

The history of science is indispensable to the philosophy of science. We cannot understand our present day concepts unless we find out how they originated and developed. The editors of this volume are keenly aware that such a history of ideas is needed rather than the 'axe-grinding' or 'dry-as-dust' method that most writers on the subject seem to employ today. We need for the whole of science what Whittaker did for the aether theories of physics. The present book falls very short of this, admittedly high, standard. One reason is that it is written by philosophers and historians; there is no scientist among the contributors. Thus the articles are not technical enough to be of much use to the philosopher of science. However, the volume provides good information for the general reader.

E. H. Hutten

Physics of the Stoics. By S. Sambursky.
Routledge and Kegan Paul, London, 1959. Pp. xi + 153. 23s.

There are two general, basic concepts in modern physics, the concept of particle and that of field. When western science began, with the Ionian *physiologoi*, the element, like Thales's water, was not yet a very precise conception. Slowly, the two rival ideas of a continuous substratum and of a discrete particle developed from this beginning. Atomism was first on the scene, while the theory of the continuum came later. The Eleatics thought of the continuum as something static, indeed making all motion impossible. The dynamic continuum, both mathematical and physical, was in the main the contribution of the Stoic philosophers.

This is described in the present book, with many quotations from the relevant sources, and there is also an appendix giving translations of some of the texts used. According to the Stoics, the universe is filled with a continuous, all-pervading sub-stratum, the *pneuma*. It holds the various parts of the universe together and through it the forces between them are transmitted. Cohesion, *hexis*, and vital tension, *tonos*, characterise the pneuma, while the earlier views of the continuum, like Aristotle's, were merely geometrical and topological. Thus, the pneuma is the forerunner of the aether and of the field of force of modern physics.

According to atomic theory, the world consists of particles moving in the void, and the causal action between them is by impact. The central concepts are 'aitia'—originally meaning guilt and, later, cause—and 'ananke' or necessity, which introduces the idea of natural law. However, random events, the famous ' swerves ' of Epicurus, may occasionally occur. The Stoic conception of causality is much stricter; and so incidentally, is their ethical code. The causal chain stretches continuously throughout space and time, from point to point, without a gap. Space-time and causality so become connected as they are in the field theory of classical physics. The strict determinism makes it possible to predict the future. ' The predictions of the diviners could not be true if Fate were not all-embracing.' It is suggestive to realise that divination was the precursor of what is called induction today.

The continuum is a more difficult concept than the atom. Atomism involves the idea of the void which seems to be more amenable to treatment than that of infinity needed by the continuum theory. The paradoxes of infinity which arise from continuity were first stated clearly by Zeno of Elea. The geometrical method of exhaustion does not really solve the problems of convergence and of limit that are involved in the paradoxes. Xenocrates, who was a teacher of Zeno the Stoic, tried to overcome the paradoxes by introducing *atomai grammai*, ' atomic lengths ', smallest quantities that are no further divisible. This is similar to the modern attempts to avoid the ' infinity catastrophe ' in field theory.

The ' inwardly infinite ' character of space, as Weyl called it, that there is always still another point between any two points, however close together they are, has remained a difficulty even to us, when the continuum is physical rather than mathematical. Though calculus helps us to conquer it mathematically, the physical meaning of infinity as a process that is never completed—as a ' symbol of creation ' as Gauss called it—still requires much elucidation.

Like Professor Sambursky's previous book, *The Physical World of the Greeks*, (*vide*, this *Journal*, 1958, **8**, 347), the present volume is a mine of historical information. All our science comes from the Greeks, and so to study the origin and early development of our concepts is a necessary task for the philosopher of science.

E. H. Hutten

History of the Royal Society. By Thomas Sprat. Edited with critical apparatus by Jackson I. Cope and Harold Whitmore Jones.
St Louis (Washington University Studies); London, Routledge and Kegan Paul, 1959. Pp. xxxii + 456 + 78. 50s. net.

This is a facsimile of the first edition (1667) of Sprat's famous work, supplemented by Introduction, full bibliographic and explanatory Notes, and Appendixes on the origins of the Royal Society, Stubbe's attack on the Society, and Hooke's 'Method of Making a History of the Weather'. In its special field, as a detailed study of an important event in seventeenth-century intellectual history, the Editors have produced a scholarly and definitive work, and their notes contain many valuable references. There is no Index.

However, for the ordinary student of the philosophy, or even of the history of science, Sprat's *History*, though often cited, has little merit, being unreliable. It was written as a defence of the Royal Society against various misrepresentations, such as the charge of impiety, and as propaganda for the new empirical philosophy by an overpatriotic Divine who regarded the 'Genius of the English Nation' as ordained to lead a Baconian utilitarian science.

L. L. Whyte

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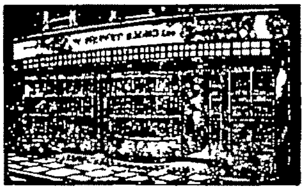

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A METHODOLOGICAL PROBLEM IN RHEOLOGY*

A. GRAHAM, G. W. SCOTT BLAIR, & R. F. J. WITHERS

I: EXPERIMENTAL EVIDENCE ON THE RHEOLOGY OF COMPLEX ALLOYS AND ITS PHILOSOPHICAL SIGNIFICANCE

A. GRAHAM

1 *Introduction*

COMPONENTS of machines, such as aircraft or gas turbines, may be made from complex alloys that contain perhaps ten elements, all of which affect their mechanical properties. In service use, the loading and temperature of the alloys vary in a rather arbitrary manner, and the many problems of designing to avoid failure in service are tackled by the industry mainly by the methods of direct development. These obviously lead to successful results, but they are expensive and laborious.

In a government research laboratory, individuals are less closely in contact with development experience than in industry and, if concerned with the problem, are at once impelled and more free to take up background research into problems of mechanical properties that are common to the class of commercial alloys with which the industry as a whole is concerned. Such a research poses the problem of providing systematic descriptions of the rheological behaviour of materials that are regarded by both industry and the academic world as being too complex for systematic and exact description.

Although the view that rheological behaviour is complex derives legitimately from the fact that it is concerned with the interplay of at least four independent variables, namely, stress, strain, time, and temperature, mechanical properties are governed by overall averages of a very large number of microscopic effects, and averages are invariably simpler than the myriad details. Moreover, certain regularities that appeared in the published results of the rather small group of engineers that have studied the mechanical properties as a subject in itself suggested that if behaviour were systematically studied in terms of these four variables it would be found to be essentially simple.[1] The chance that appearances were genuine has led the author and his colleagues to spend about ten years in unravelling a tangled skein of regularity and to reach and support the conclusion that mechanical behaviour is no less ordered than other and better-understood aspects of nature. Acknowledgement is especially due to the contribution of Mr K. F. A. Walles.

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* Symposium at the Conference of Rheology, Manchester, 6–8 April, 1960

The method followed has been the converse of that which is customary. Instead of progressively narrowing down and idealising the field of interest in direct pursuit of exact ideas, a search has been made for trends over the widest practicable extension of the field. A general mathematical principle has been progressively circumscribed to express the trends. The regularities found are of a rather unexpected nature, and it is not perhaps surprising that the work has been labelled as ' merely empirical ' and without significance on one assessment and as ' academic and artificial ' on another. In the present paper the nature of the regularities and of the experimental support for them are illustrated, and a discussion is offered of the fallacies of such general assessments.

2 *Results*

Certain ideas in the macro-rheology of metals run very close to ideas that have been put forward and developed in the field of non-metals by Dr G. W. Scott Blair and his colleagues. The present work stems from this double root, and it supports and extends a number of his conclusions. The regularities centre about an equation

$$\epsilon = C\sigma^{\beta}t^{\kappa}$$

put forward by Nutting in 1921 to represent the creep strain ϵ of pitch at time t when loaded with a constant stress σ. The quantities β and κ are numerical constants and C depends upon the temperature.

In order to take account of the experimental fact that behaviour cannot in general be represented by an algebraic equation of this kind, for the strain depends not only upon the instantaneous stress but also upon the way the stress has previously varied with time, the Nutting equation needs to be regarded[1] as an integrated form of a more general equation such as

---

[1] A. Graham, ' Phenomenological Theories of Creep ', *The Engineer*, 1952, **193**, 198, 234; ' The Phenomenological Method in Rheolgy ', *Research*, 1953, **6**, 92

$$\epsilon = \int_0^{t_1} (t_1 - t)^\kappa \frac{d\sigma^\beta}{dt} dt$$

in which t_1 is a particular value of t. The equation is thereby made invariant with respect to time. The equation is allied to the fractional form[1] that Scott Blair has advocated. It represents an addition of elementary 'creep processes' which originate individually, with each increment of (σ^β), at the instant each such increment occurs. Each contributes thereafter a progressively changing increment of strain.

For particular 'paths of loading', with σ as a function of t, as for example in ideal creep testing with stress constant, the equation reduces to the Nutting equation with an appropriate constant factor. The experimental support for this result shows that the use of an algebraic equation is legitimate when the loading path is fixed.

If attention is now confined to a particular loading path, the strain is not in general represented by a single Nutting term, but by the sum of a series of Nutting terms, thus

$$\epsilon = C_1 \sigma^{\beta_1} t^{\kappa_1} + C_2 \sigma^{\beta_2} t^{\kappa_2} + \dots$$

The regularities of especial interest, which the detailed studies have rather fully confirmed,[2] is that the exponents κ and β take only particular values. When the stress is constant for example, the exponents κ take values only from a sequence based upon the powers of 3, namely

$$\dots \left(\tfrac{1}{9}\right), \tfrac{1}{3}, 1, 3, (9), \dots$$

and the ratios of κ to β take values only from a sequence based upon the powers of 2, namely,

$$1, \tfrac{1}{2}, \tfrac{1}{4}, \tfrac{1}{8}, \tfrac{1}{16}, \dots$$

This algebraic formula is a straightforward inference from experimental results. The general formula contains a sum of integrals like the right-hand member of the second equation with the appropriate fractional values of κ and κ/β.

These regularities are seldom manifest immediately in experimental data on account of its scatter, limited range, and generally poor sampling, and because the effects of more than one term are generally superimposed. In favourable circumstances one or two terms predominate over the remainder: the fractional exponents are then closely displayed. In other circumstances, their validity must be established by detailed analyses joined to an assessment of significance in terms of the confidence limits of the data.[1]

---

[1] A. Graham, G. W. Scott Blair, Discussion of paper by H. McCallion and D. M. Davies, *Proc. Inst. Mechanical Engrs.* 1955, **169**, 1135, 1137

[2] A. Graham, and K. F. A. Walles, 'Regularities in Creep and Hot Gatigue Data', *Aeronautical Research Council Current Papers*, 1958, C.P. 379, 380

The crucial test[1] has been to show that the laws lead to successful extrapolations of creep data over considerable ranges in time, for different sets of data grouped together to provide significant samples.

It appears from this work that the complexities of mechanical properties have less to do with the complexities of composition than with the presence of four variables and with the considerable range of these variables over which the pattern of regularity is extended. The formula can best be regarded as offering a rather high ratio of experimental facts co-ordinated to hypotheses made; the two series of standard exponents however appear to be, in some sense, fundamental.

3 '*Fundamental*' *versus* '*Empirical*'

These conclusions are opposed to the view that it is almost inconceivable that all the complex phenomena concerned can have any simple explanation; that the phenomenological method that has been followed is mere curve-fitting without significance; that merely to represent results by a formula does not meet the essential need of explanation; that work on commercial alloys is necessarily empirical; that progress can only be made by fundamental studies; that only work on pure metals and simple alloys can be fundamental; that a fundamental result must be based upon atomic factors; that fractional power laws are merely empirical; that only integral powers and exponentials can be fundamental; and so on. The list is probably long enough for illustration.

Most of these problems centre on the significance of the term 'fundamental'; and they appear to involve old controversies that have been well fought over in the past. The term 'fundamental' has clearly both a scientific and a metaphysical connotation. Scientifically, a fundamental result or principle is one that is common to a wide range of experimental conditions and on the basis of which a wide range of results may be predicted. A metaphysical connotation is involved when the common principle is regarded as a fact of nature and as an absolute truth.

If one adopts the scientific viewpoint, then an important consideration is that whether a result is empirical or fundamental depends entirely upon the point of view. An experimental uniformity of behaviour such as the present can be regarded either as an empirical target for prediction on the basis of hypotheses made on the atomic scale of magnitudes, or as offering itself the fundamental hypotheses for some field theory of the macroscopic scale of magnitudes. Those who are primarily interested in atomic phenomena are thus liable to make the opposite assessment to those who, like the author, are concerned with behaviour on the scale of engineering. A great deal of confusion can arise from the fact that the two groups of research workers stand back to back, on a common result, and face in opposite directions.

---

[1] Work to be published

Another source of confusion can arise from the fact that any science has two distinct phases with an essentially inverse relationship between them, namely, a formative phase and a development phase. The formative phase consists of inductive, analytical, and often disjointed detective work, in which one possibility is tried after another in a search for unifying principles and basic elements. It is not strictly logical in a classical sense and involves working inwards, from an imagined generality spread over a range of personal experience, along the lines of greatest probability, towards a centre.

When success is achieved, however, there is a complete change of procedure. The stage of formal development then follows in which the principles and elements are taken *by definition* as fundamental, and the science then develops outward and backward along the general course by which it came.

A great deal of misunderstanding can arise between those who are working outwards from familiar basic principles they accept without question and those who work inwards also, and are prepared to entertain some adaptation of basic principles. The necessity for such an adaptation in rheology is clear from the fact that, as Scott Blair has pointed out, some rheological properties are not physical properties in the ordinary sense of the term, for they are altered by the process of measurement.

Another source of difficulty is the canonisation of hypotheses: a promotion of respected hypotheses that have successfully co-ordinated a limited range of experience to the status of fundamental truths that must be preserved inviolate throughout all reasoning. In the present context, the hypotheses that appear to have been canonised are those that give rise to exponential laws and integral power laws. Canonisation evidently arises in the transference of ideas from master to pupil.

It inhibits thought because, in comparison, every other consideration is made trivial. Experience shows of course that the most revered hypothesis of one age and field of study turns out later to be only a limited part, or special case, of a wider concept.

A tendency to canonise is particularly associated with the concept of purity. The view exists, although fortunately it is dying, that work on pure materials is more scientific—more valid—than work on everyday materials. It is evidently a vestigial survival from the time when those who would now be called scientists studied the heavens, which were believed to be perfect and unchangeable, and tried to relate the best on this earth to the infinite. The mud, cheese, and metal, of present day rheology were earth-bound and profane. The present results cast doubt upon whether purity as such has any rheological significance.

4 *A philosophy of the Phenomenological Method*

The philosophy of the early days of quantum mechanics, of the Jeans-Schrodinger-Eddington-Stebbing period, appears to be a valuable guide to the labour of developing rheology from a craft into a science. One of the lessons that was learned with great difficulty, but all the more firmly in that period was the futility of attempting to visualise, in any final detail, the ultimate structure of matter in terms of the familiar pushes and pulls, billiard balls, and so called realities of everyday experience. Atoms, electrons, and the like were derived concepts, and had the status of artefacts which were not to be accorded any real existence in their own right. The business of exact science was to deal in observables alone—with the numerical results of experiment—and to specify numerically the experimental relationships between observables. A scientific theory consisted of mathematical symbols, operational rules for transforming one symbol into another, and a set of rules for interpreting results in terms of observables. Physical pictures and models were either scaffolding or metaphysics.

Whether this is the only valid workaday philosophy of science is a matter for debate, but at least it enabled atomic theory to escape from the trammels of its anthropomorphic origins. It is also the philosophy of the phenomenological method that has led to the present results. The philosophy is especially rejected by those who are fascinated by the various physical events that may be seen to happen before their very eyes; but the visual-model approach appears to belong to the early formative period of enquiry. The quantum-mechanical philosophy appears to be adopted without difficulty in the later development period. Those who describe the rheology of metals in terms of patterns of dislocations, for example, readily transfer to the highly-formal theory of elasticity and Newton's laws of motion. The work here in review shows that progress can be made by a direct search for the mathematical expressions that adequately represent a class of experimental graph, and for the mathematical operations that enable passage to be made from one experimental graph to another.

To attempt to rank this procedure on a fundamental versus empirical evaluation is to obliterate all discussion with the metaphysical positive and negative infinities of a ' fundamental truth ' and ' merely empirical device '; and it is quite unscientific.

5 *An application of the Phenomenological Method*

The view that research is a study of relationships rather than a pursuit of absolute truth permits a greater mobility of speculation. For example, the second equation

$$\epsilon = \int_0^{t_1} (t_1 - t)^\kappa \frac{d\sigma^\beta}{dt}\, dt$$

is a particular case of the general principle of linear superposition according to which any behaviour or function may be regarded as being composed of a distributed sum of repetitions of a unit behaviour or function treated as indivisible entities on a smaller scale of magnitudes. The ' unit function ', t^κ in the above expression, may be any physical function whatever, and the ' unit cause ' $(d\sigma^\beta/dt)\, dt$ may be any physical cause. The situation in nature to which the equation corresponds appears at first sight to be more akin to biological growth than to inanimate physical change—more akin to the growth of an organism by a progressive aggregation of parts with each part itself growing and changing in a uniform and characteristic manner. The distinction between biological growth and inanimate physical change is unlikely to be a genuine one, however, for rheology and biology are both concerned with regroupings and rearrangements of the same kinds of atoms. The biological parallel illustrates in passing the common misconception that worthwhile and reliable theories can only be built up from consideration of the behaviour of atoms.

The biological parallel is by no means fanciful or irrelevant. While discussing with Dr Scott Blair the mathematical bases of the power law, he drew my attention to an argument in a paper by von Bertalanffy which appears to have originated with Julian Huxley. Huxley was interested in the ratio of sizes of two parts of the same growing animal; and he showed that if two independent quantities each growing exponentially are compared, then the ratio of their magnitudes at any instant follows a power relation. He supported the biological power law of relative growth by many examples from different animals. A simple adaptation of his argument leads to a similar sequence of exponents to those encountered, as previously mentioned, in the study of complex alloys.

In the comparisons of body size that led him to the power law, Huxley was concerned with the relative size of one finite part of an animal, say a claw, with the size of another finite part, say the shell, each being regarded as a uniformly-growing whole. He then discusses the case in which there is a progressive change of growth distributed over the animal, and quotes D'Arcy Thompson's observation that the forms of different species of fish or of different species of crab are geometrically related to one another by a regular Cartesian transformation. It appears, as Huxely remarks, that the different species have diverged in their evolution by the differing development of growth tendencies centred in different parts of the animal.

The use that Huxley and his successors have made of this reasoning may have a special rheological significance. The results offer a mathematical analogue, and perhaps even an actual equivalence in a chemical sense, between the deformation of an inanimate body in a mechanical environment and the changing form of a developing organism in a biological environment.

Ministry of Aviation
National Gas Turbine Establishment
Farnborough

II: NEW OUTLOOKS IN RHEOLOGY *

G. W. SCOTT BLAIR

'There is no escape from philosophy. The question is only whether a philosophy is conscious or not, whether it is good or bad, muddled or

---

* I am indebted to Mr A. Graham, Mr R. F. J. Withers, Professor M. Reiner and Professor A. Katchalsky for helpful discussions; and to Dr P. White for information about the history of the definition of fractional differential coefficients.

> clear. Anyone who rejects philosophy is unconsciously practising a philosophy.'[1]

INDEED it seems that those physicists who most hotly deny having any philosophy are inclined to to be the first to condemn new ideas which contravene their own metaphysical presuppositions and such prejudices serve as hindrances to the development of the younger branches of physics.

Although the mechanistic beliefs of the nineteenth century are no longer accepted consciously, many physicists have not yet fully grasped the implications of the change in outlook. As Bridgman[2] has put it:

> Many will discover in themselves a longing for mechanical explanation which has all the tenacity of original sin . . . Just as the old monks struggled to subdue the flesh, the physicist struggles to subdue this sometimes nearly irresistible, but perfectly unjustifiable desire.

The new outlook has been well expressed by Johnson[3]:

> If symbols however indeterminate can be fitted together in functional form and in equations predicting what actually happens, we regard with tolerant but superior agnosticism our ancestors' desire to label those symbols and attach them to pictures of 'things' behaving as little miniatures of large-scale characters or bodies.

But many physicists have hardly yet reached this stage.

We have no precise definition for science as a whole, but for a description of the scope of intelligent scientific activity, one might well combine two well-known definitions, the one of intelligence and the other of science. Science is concerned with perceiving relations between those elements of our experience which are common or potentially common to all normal people.

Rheology is a branch of physics which deals with the deformation and flow of matter. As in other branches of physical science, there are four principal kinds of relation which we try to establish.

(*a*) *Relations between the behaviour of gross matter and that of its molecular or atomic constituents.* Rubber is believed to contract elastically because of its long chain molecules, pivoted to rotate at a certain angle, and buffeted about by neighbouring molecules into chance configurations. This model enables us to calculate the elastic behaviour of rubber at various temperatures. In a sense, this model is like the system of wheels in Mr Withers's black box (see p. 282), but in this case (*a*), we really believe that there are wheels, just as we believe that there are invisible genes which form parts of the visible chromosomes. We hope to get more and more evidence for their real existence as science progresses.

---

[1] K. Jaspers, *The Way to Wisdom*, London, 1951

[2] P. W. Bridgman, *The Logic of Modern Physics*, New York, 1928

[3] M. Johnson, *Time, Knowledge and the Nebulae*, London, 1945, p. 160

The rheological behaviour of gross matter usually depends on its molecular and atomic structure, so this type of relation is most valuable. In rheology, however, it is not always possible to find such relations, nor is it right to assume that all aspects of macrorheological behaviour are always entirely dependent on molecular or atomic structure. As an extreme case, two samples of sand, chemically identical, will behave quite differently when subjected to the same stresses if their particle size distributions differ appreciably. The fineness of grinding of clays much affects their plasticity. Here the causes may be partly at a molecular level, though certainly not entirely so.

(*b*) *Relationships between the behaviour of gross matter and that of other simple systems (models) which follow approximately the same equations, where no causal link exists.* We can observe light only when it is associated with matter and, in certain experiments, light then behaves like a wave. In certain experiments milk curd behaves like a particular system of viscous and elastic elements called, by rheologists, 'a Burgers Body' and shown in Figure 1 (see Scott Blair and Burnett[1]).

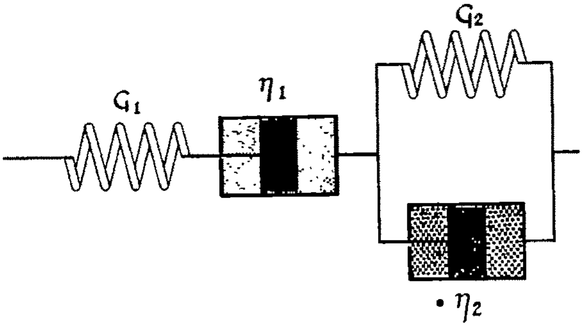

FIG. 1

An electric clock behaves like my watch, with all its wheels, but we do not expect the find to same mechanism within the clock. A failure in a distant power station will stop the clock but will not affect my watch. There are relations between light and waves; curd and Burgers Bodies; and the pointers on the black box, and the wheels in my watch.

---

[1] G. W. Scott Blair, and J. Burnett, *Brit. J. Appl. Phys*, 1959, 10, 15

(*c*) *Relationships between phenomena which at first sight appear quite unconnected*, but which are, in fact, linked, although no 'cause' is known: e.g. the forces which keep planets in their orbits and those that cause apples to fall to the earth. Such relationships are not common in rheology.

(*d*) *Relationships between the behaviour of matter under varying intensities of the same or comparable physical stimuli.* Under certain conditions, if we increase the stress on a milk gel, the strain which recovers when the stress is removed will increase proportionally.[1] Hence, as Mr Withers points out, we can calculate a useful rigidity modulus from many different observations. Under other conditions, the rate of strain is proportional to stress and a viscosity is a useful and meaningful entity.

Some rheologists dogmatically believe that one or other of these, generally (*a*) and (*b*), are the only 'sound' types of relation in our Science.[2] Enormously valuable as their contributions have been, this is just not true. Few rheologists stick entirely to type (*a*)—there are far too many phenomena for which an explanation cannot be offered in molecular terms. Type (*b*) explanations are accepted so long as the 'wheels' consist of ideally elastic (Hookean) springs, perfectly viscous (Newtonian) dashpots [3] and (St Venant) sliders, representing static friction. Nor is one supposed to believe that these 'parts' really do exist. This is similar to the situation with respect to some of the complicated machinery of pre-Copernican cosmology. As with epicycles, so with dashpots and springs: if we postulate enough of them, we can always account for any experimental situation.

Spring and dashpot models lead to exponential equations, which, in the simplest cases, means that stresses will relax at constant strain and strains will build up under constant stress at rates proportional to themselves at any moment of time. The rates do not depend on the earlier stages of the experiment, since the moduli and viscosities remain constant. If, in fact, in more complex materials, the process of stressing or straining is altering the values of these properties, the simplest assumption would surely be to suppose that, in straining under constant stress, for example, the rate of strain, $d\epsilon/dt$,[1] would be proportional not to the strain at time t but to the average rate of strain since the time when the straining started, or

---

[1] Ibid.

[2] Two American authors have recently shown much concern that they are not able completely to describe the behaviour of sand-water mixtures in terms of atomic arrangements. See W. A. Weil and W. C. Ormsby in *Rheology: Theory and Applications III*, Ed. F. R. Eirich, New York 1960, Ch. 8.

[3] A 'dashpot' is a damping device consisting of a piston operating in a cylinder of viscous oil.

$$\frac{d\epsilon}{dt} \propto \frac{\epsilon}{t}$$

and this is a power equation. Writing σ for the constant stress and k for a fractional exponent (normally $1 > k > 0$) we have:

$$\epsilon = A\sigma t^k$$

and this equation does not give invariant magnitudes for simple properties like viscosities and elastic moduli. Such a ' model ' cannot be precisely expressed in terms of springs with Hookean elastic moduli and dashpots with Newtonian viscosities. For a Hookean spring, $\sigma \propto \epsilon$, and for a dashpot, $\sigma \propto \frac{d\epsilon}{dt}$. For the many complex materials which follow power equations, we can write $\sigma \propto \frac{d^\mu\epsilon}{dt^\mu}$ where $1 > \mu > 0$.

Let us compare this type of treatment with the classical exponential treatment. The simplest general model to consider is that of Burgers, as shown in Figure 1. We may consider either the relaxation of stress at constant strain, or retardation of straining under constant stress.[2] Let us consider the former and plot the viscosities of the dashpots in the ' series ' part of the model (η_1) against the moduli (G_1) of the springs.

Consider any one sample, X, of a set X, Y, Z. (Fig. 2a). This is placed so that the cartesian co-ordinates represent its shear modulus (G) and its viscosity (η) respectively. If we join X to the origin, then $\tan\theta$ will represent its relaxation time. (We can now leave out the suffixes, since the argument applies equally well to retardation times.)

Now consider a set A, B, C, of much more complex materials, whose behaviour cannot be easily represented by dashpot-spring models. Figure 2b, though superficially similar to 2a, is really very different. Again, one axis represents (*stress*, σ)/(*rate of shear*, $d\epsilon/dt$) and the other (*stress*, σ)/(*amount of shear*, ϵ), but here the co-ordinates are polar. Viscous liquids may be shown as points along the vertical axis and elastic solids as points along the horizontal axis, at distances from the origin which represent the magnitudes of η and G respectively. The points A, B; C, which represent complex systems, are placed in such a way that the angle ω made by the line joining them to the origin represents the position of the sample in a continuum lying between the viscous condition and the elastic condition. The radius vector OA is analogous both to η and to G, for if we represent it by χ we can write

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[1] For the sake of simplicity, the same symbols are used for tensile and shear stresses and likewise for strains. These also conform to the usage in Mr. Graham's paper. This should be easier for the non-rheologist reader, though it is not to be recommended for strictly rheological papers.

[2] The ratio η_1/G_1 is called the ' relaxation time ' that of η_2/G_2, the ' retardation time '.

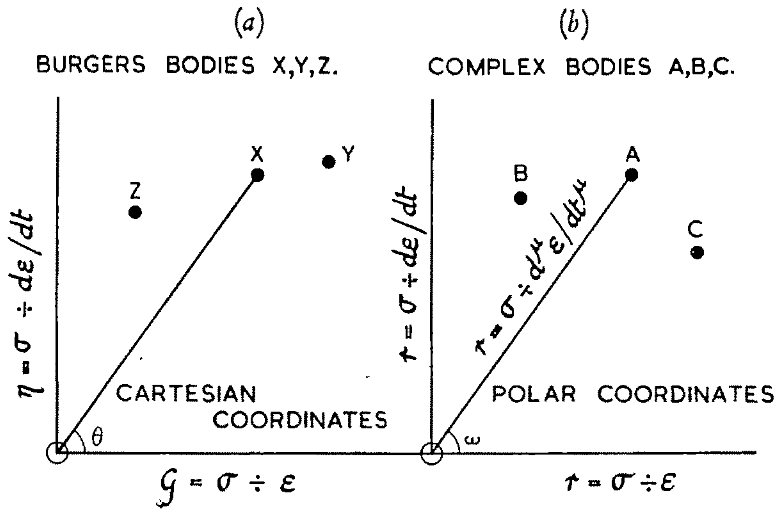
(a)
BURGERS BODIES X,Y,Z.
(b)
COMPLEX BODIES A,B,C.
X
Y
Z
$\eta = \sigma \div d\epsilon/dt$
θ
CARTESIAN COORDINATES
$G = \sigma \div \epsilon$
A
B
C
$f = \sigma \div d\epsilon/dt$
$\chi = \sigma \div d^{\mu}\epsilon/dt^{\mu}$
ω
POLAR COORDINATES
$f = \sigma \div \epsilon$

FIG. 2

For viscosity: $\eta = \sigma \div d^1\epsilon/dt^1$ or $\sigma \div d\epsilon/dt$
for elasticity: $G = \sigma \div d^0\epsilon/dt^0$ or $\sigma \div \epsilon$
for complex
systems: $\chi = \sigma \div d^{\mu}\epsilon/dt^{\mu}$ where $1 > \mu > 0$.

It is clear that the value of μ will depend on sin ω, varying from zero for an elastic solid to unity for a viscous fluid. Thus χ is intermediate between η and G, in the sense that the differentiation will be done μ times, where $1 > \mu > 0$. Furthermore, the length of OA, which represents the second polar co-ordinate, will represent the intensity of the property, χ, which approximates to viscosity when sin $\omega \approx 90°$ and to an elastic modulus when sin $\omega \approx 0°$. It will therefore have dimensions $ML^{-1}T^{-(2-k)}$, such that $k = 1$ for liquids and $k = 0$ for solids, and has intermediate values for the complex materials.

Now it is clear that, if we wish to compare, say A and B, we cannot just say $\chi_A > \chi_B$ as we might say $\eta_A > \eta_B$, or $G_A > G_B$. This would constitute the error in dimensions of which we are sometimes accused, but there would seem to be no record of any rheologist making such a mistake. We can, of course, compare directly any set of samples having the same value of k (say 0·2, which happens to be a common value for high polymers). In other cases, we must always compare samples in terms of both k and χ taken together, i.e. of the position of the points on the diagram. This is a novel idea, but in no way an unsound one. Such samples are too complex in their behaviour to be compared along a single scale. Either they must be defined by a very large and perhaps infinite number of ' physical properties ', or they may be compared in terms of their relative positions in a diagram such as that shown in Fig. 2b. If strain and stress are not linearly related so that a stress exponent is also needed, they are defined by means of their position within solid figures.[1]

This method of treating certain data has two principal advantages. First, in many cases, as Scott Blair and others have shown [2,3], in order to obtain comparable goodness of fit, considerably fewer parameters are needed than if a series of exponential terms is used. Secondly, a much closer link is established in this way, with psycho-physical studies of the assessment of rheological properties of industrial materials by handling them. Such subjective assessments will long continue to be of importance in many industries. An earlier account of these ideas was given in a paper in this journal some years ago.[4] Criticisms on dimensional grounds were ably refuted, at about the same time, by Dingle.[5]

A criticism of such methods more serious than that of dimensional inhomogeneity, concerns the definition of differential coefficients of fractional order. A concept of this kind must be precisely definable. One might start, for example, by trying to define the operation $d^{\frac{1}{3}}\gamma/dt^{\frac{1}{3}}$ such that, when carried out three times, the result is the same as a single differentiation. But we must then show that the result of applying such an operation is unique, or at least leads to only a limited number of alternatives; and this is not the case. It is better to follow Riemann's integral form of definition (inherent in Mr Graham's treatment) which gives a perfectly consistent and intelligible result for any real fractional order, rational or irrational.

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[1] G. W. Scott Blair and B. C. Veinoglou, *J. Sci. Instrum.*, 1944, **21**, 149

[2] G. W. Scott Blair, B. C. Veinoglou, and J. E. Caffyn, *Proc Roy. Soc.* (*A*), 1947, **189**, 69

[3] G. W. Scott Blair, and J. E. Caffyn, *Phil. Mag.*, 1949, **40**, 80

[4] G. W. Scott Blair, this *Journal*, 1950, **1**, 230

[5] H. Dingle, *Phil. Mag.*, 1949, **40**, 94

Although it is harder to envisage differentiating, say, $1/\pi$ times than to imagine a half, or a third of a differentiation, one must assume that the space in Figure 2(b) forms a continuum. (This is a point which was raised by Dedekind.) If we suppose a line, corresponding to a set of samples having the same distances from the origin starting at $\sin \omega = 0$ (elastic solid) and gradually rotating towards $\sin \omega = 1$ (liquid), this line will pass continuously through all intermediate angles, not only those corresponding to rational fractions. It is unthinkable that there will be some kind of 'exclusion principle' forbidding specified angles.

The idea of comparing the behaviour of complex bodies in terms not of distances along a line (a scale), but by their positions in a space of two or more dimensions, is worth further consideration. But it is clear that such treatments will not lead us to simple models of rheological behaviour!

It has been objected that the quantities here proposed for the specification of the rheological behaviour of certain materials are not tensorial and, therefore, do not have the invariance required of fundamental physical properties. This, of course, depends on what is meant by 'fundamental physical properties'. As a concession, I have felt it best to describe such entities as 'quasi-properties'; though I am not sure whether this concession was really a wise one. There are many situations in physics for which the tensorial type of invariance is highly advantageous, but I cannot see that establishing relations between the rheological behaviour of complex materials is one of them. As an illustration, some elastic materials do not obey Hooke's law over any appreciable range of straining, but a power relation is found to hold between stress and strain (Bach's equation). It is clear that this power equation will not be valid for very small strains, when Hooke's law is probably obeyed. But this in no way precludes the use of Bach's equation for all but the smallest strains. Another case is the fairly wide use now being made of Casson's equation[1] which gives a linear relation between square-root of stress and square root of shear-rate in material flowing through a capillary, or between concentric cylinders. This does not allow of a reversal in the direction of the flow.

I would conclude with a word about Mr Withers's type (1) and type (2) explanations (see p. 283). I take it that what I have called (*a*) and (*b*) relations offer type (2) explanations, whereas (*c*) and (*d*) are of type (1). Sometimes the question whether a model is supposed to represent real components of a system, is left open; but, as time goes on, the tendency is more and more against reification of such models.

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[1] N. Casson, in *The Rheology of Disperse Systems*, Ed. C. C. Mill, London 1959, Ch. 5

Mr Withers asks if there are any hopes of finding some explanation of type (2) class, i.e. perceiving relations of my types (*a*) or (*b*) with power laws and fractional differential coefficients. He agrees with me that this is not *essential* and I agree with him that type (2) explanations have proved so valuable in the past that it would be well worth trying.

I feel that Mr Graham's contribution comes nearest to making such a link. I feel pretty sure that if we ever succeed in finding an explanation of the type (2) class for the power equations, it will come through the simple mathematical fact that a power equation results from the combination of two exponentials; but I cannot yet see just how this is to be done.

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III: EXPLANATIONS AND MODELS IN SCIENTIFIC THEORY CONSTRUCTION

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1 *Introduction*

THE work of Graham and Scott Blair has been criticised in the past on various grounds. Graham has enumerated some of these objections. I want to concentrate on a few of them:

(1) That it is inconceivable that all complex phenomena can have a simple explanation.
(2) That the phenomenological method that has been followed is mere curve-fitting without significance.
(3) That merely to represent results by a formula does not meet the essential need of explanation.
(4) That progress can only be made by fundamental studies.
(5) That a fundamental result must be based upon atomic factors.

These criticisms suggest to me that a discussion of the rôle of generalisation and explanation in science might be useful with respect to their work and this will also involve a discussion on the rôle of models in scientific theory construction.

My impression is that whenever a controversy over scientific statements arises a close examination of the type of statement being discussed and a study of the statement's exact position in the ordered hierarchy of scientific statements focuses the discussion on to the way that that statement is functioning within the scientific theory. To do this is always a profitable pursuit and is one of the main functions of the philosopher of science.

2 *Sketch of Statements in a Rheological Theory*

In order to arrive at some idea of the rôle played by the extension of the Nutting equation in rheology I shall consider a statement of its function as expressed by Scott Blair and Burnett in a paper[1] they published in 1959. I shall classify the statements made in that paper under the scheme described by Woodger.[2]

Scott Blair and Burnett reported *observation records* about what happens when small pressures are applied to one end of a column of renneted milk (' curd ') in a U-tube and the displacements are magnified a hundred fold by means of an alcohol index in a horizontal capillary. At the first stage of the discussion no theoretical terms seem to enter. However, soon the authors calculated the overall shear modulus G. This we are told is calculated from the movement of the alcohol index of recovery h, the density of the manometer liquid ρ, the radius of the U-tube R; the length of the column of gel L and the radius of the recorder capillary a, from the equation

$$G = \frac{R^4 \rho g}{8La^2} \cdot \frac{P}{h}$$

The overall G can be calculated from an experiment in which the sample is loaded for, say, one minute then allowed to recover for, say, two minutes. The whole of the recovery, almost complete in two minutes, gives us h.

In the second type of experiment readings are taken at frequent intervals during the two minutes of recovery and so they calculate a series of unrecovered displacements. This enables G to be divided into two parts, immediate and slow.

Now it is obvious that more is involved in this procedure than mere observation. The equation from which G has been calculated must represent some other type of statement in which G is a definition for a complex relationship. Certain of the terms in the equation are also definitions of other relationships. My remarks about G will be applicable to these terms.

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[1] G. W. Scott Blair and J. Burnett, ' On the Creep, Recovery, Relaxation and Elastic " Memory " of some Renneted Milk Gels ', *Brit. J. App. Phys.*, 1959, **10**, 15-20

[2] J. H. Woodger, *Biology and Language*, Cambridge, 1952, Chs. 1 and 2

The definition of G is based on a number of observations which suggest a general relationship, a generalisation of observation records, expressed by the right-hand side of the equation which is believed to exist in a repeatable fashion. The relationship is sufficiently important for it to be abbreviated to the single letter G and to be known as the shear modulus.

The authors go on to describe the result of calculating four parameters: the immediate modulus, the delayed modulus, the internal damping viscosity and the external viscosity. They notice that these parameters 'behave approximately as a Burgers body'. It seems to me that the impetus to discovering this behaviour must have come from the use of a model, that of springs and dashpots, suggesting the making of observations about the four parameters that were calculated. The results obtained were the test of the spring and dashpot model, where this was functioning as an *explanatory hypothesis* suggesting the making of fresh observations. Let us, therefore, consider what is involved in inventing an explanatory hypothesis.

If we consider the sort of statements that form explanatory hypotheses we can see that there are two main types. Let us consider a simple example of a situation requiring explanation. We have a black box with a dial on the front having numerals at various points and two hands which rotate at different speeds. Can we devise an explanation for the behaviour of the hands and in particular can we predict when the hands will coincide and can we perhaps give an estimate of the times on our watches when the coinciding will occur? A scientist could look for an explanation of the behaviour of the hands in two ways.

(i) He could calculate the angular velocity of both hands and work out an expression relating the two in such a way that the required prediction can be made. This expression would constitute in one sense an explanation of the behaviour of the hands (I am going to call it a *Type 1 explanation*). This explanation could tell one a lot about the movements of the two hands and could predict various relationships between them. The question I think that might be considered is whether the extension of the Nutting Equation is an example of a *Type 1 explanation.*

(ii) Or, a scientist could first remember that he has seen other systems, such as the one he has in the watch on his wrist which he knows contains cogs, balance wheels, springs, and so forth. Using this as a conceptual model he could then invent statements about the presence of such entities inside the black box, and their relationships to one another and their relationships, causally, to the movement of the hands. From these statements he could predict the behaviour of the hands as before. He could also predict, for example, the existence of cogs, etc., as the parts of the black box system. He could make predictions about the number of teeth on the cogs which he suggests exist in the black box and he could test these predictions by opening the black box. Notice what he has done. He has used his own watch as a model system and the statements about the parts of his watch he has transferred to the black box system. This may be successful in predicting the behaviour of the hands but it may be falsified if he finds no cogs inside the box. It may, for example, turn out that the black box is run by electricity using an alternating current. But I suggest that until he has tested his theory his statements about the hypothetical cogs will give him reasonable explanation of the behaviour of the hands on the front of the black box. In other words—it is the same as a *Type 1 explanation.* He can only test the complete adequacy of the model in terms of the predictions of the cogs and other parts.

Another thing to notice is that this type of explanation involves inventing the existence of parts of the black box and their hypothetical relationships and it is because of the invention of these parts that totally *new* observations can be made. Let us call this type of explanatory hypothesis a *Type 2 explanation.* This is a convenient point to indicate that there is a fourth type of statement found in science, *consequences of explanatory hypothesis*, i.e. statements about the hypothetical cogs.

I do not want to discuss the merits of either type[1] of explanation, but is it only fair to say that most scientific theories progress along the lines of *Type 2 explanations.* The behaviour of chemicals in a test tube is explained by the statements about molecules. These can be explained in terms of statements about atoms and valency and these can be explained in terms of statements about electrons and protons, so that one arrives, in a well developed science, at a theory with several layers of *Type 2 explanatory hypotheses* from which an original observation record is derivable and from which new types of observation records can be made.

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[1] A somewhat similar analysis of explanatory statements has been made by B. Ellis, ' A Comparison of Process and Non-process Theories in the Physical Sciences ', this *Journal*, 1957, 8, 45-56.

3 *The Use of Models in Science* [1]

In the example I have just given we used the model of our own wrist watch for one reason. This was because we have a description of its behaviour ready to hand as a calculus. The calculus consists of statements which have been tested in the past and found satisfactory for our watches. The calculus itself will consist of statements expressing relationships between variables and in order to derive consequences from it which can be tested those variables must be replaced by subject matter signs. In other words the calculus must be given a semantical interpretation *at least* and only *at least* at the lower levels of the calculus. It must have a semantical interpretation at those levels where we wish to test it. However, when we use this calculus as a model the first problem that crops up is whether the semantical interpretation, appropriate to the sciences of wrist watches, is suitable for the new situation. In the case of the black box being an electric clock it will soon turn out that the interpretation is falsified. Note that it is the interpretation which is incorrect—not the calculus—for that enables us to predict. We are then faced with the choice of looking for a new interpretation of the variables in the calculus or trying to think up a new calculus with its own interpretation.

In any case, the important point is that the model was used in the first place because it had its own calculus, and it was a system of ready to hand statements that we were looking for. Provided we can invent a calculus we do not *need* to look for a model. In practice it is difficult to invent calculi, and the heuristic desirability of models for supplying calculi has unfortunately changed the emphasis from realising the importance of the calculus to believing the importance of the model. The calculus is the most important thing for the development of a theory, the model—or rather the semantical interpretation of a model—may actually be misleading as in the billiard ball model for fundamental particles.

Now a danger in the development of a science is that a good science may have a well established series of layers of explanatory hypotheses of *Type 2*. Because they are established they fit most fields and have been successful in the past. If a new field of observation records arises the generalisations of them may be arranged as the *consequences* of using as a model calculus an established theory which has been successful and is interpreted in another field. Provided that there are no counter-examples, progress proceeds by taking over the old theory as a model into a new field and looking for entities in the new field which behave like the parts at the various levels in the old field. In other words, we attempt a new interpretation of the calculus by looking for new subject matter signs to replace the variables in the model calculus.

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[1] A more detailed account of the use of models in science is to be found in E. H. Hutten, 'The Role of Models in Physics', This *Journal*, 1954, 4, 284-301, and M. B. Hesse, 'Models in Physics', *this Journal*, 1954, 4, 198-214.

If counter-instances are produced or if predictions using the old theory do not quite fit the generalisations of observation records in the new theory an attempt can be made to modify the model. Generalisations of accurate observation records in the new field may give rise to discrepancies when tested if the old theory is used to interpret or derive them. Further, when one has a satisfactory system this can be sufficiently powerful for it to dictate the sort of generalisation that is made. If this is the case and counter-instances are still produced specific *ad hoc* hypotheses can be invented to cover the discrepancies from experience. In the end the calculus becomes extremely cluttered up with these *ad hoc* hypotheses.

Further, a situation may arise in which a scientist has attempted a *Type 1 explanation* which works with greater elegance and greater precision in prediction. In this case the problem becomes acute if the *Type 1 explanation* is not derivable from the older theory used as a model. The scientist is then faced with the problem of whether to use the *Type 1 explanation*, because of its efficiency, empirically, to abandon it, or whether to look round for another model which might help him to invent *Type 2 explanation* from which this particular *Type 1 explanation* is derivable. So far as I can see there is no reason in principle why this should not be attempted, other than the usefulness of the old model and the natural conservatism of scientists. If it were achieved it may involve the over-throw of a pre-existing theory. However, it is worthwhile remembering that it is only a model calculus that is being over-thrown and the process may possibly end up by being responsible for a more adequate theory. The new theory incidentally should have as one of its consequences some of the successful statements of the old model. This is the way in which real progress in science is achieved.

4 *Discussion of the Nutting Equation in Rheology*

Let us now examine the situation as seen in the Scott Blair and Burnett paper which reflects the present position. The authors say in the discussion that they have found it convenient to *describe* their experiments in terms of models of dashpots and springs. It is worth noting that by description here they mean that they have *explained* their results in terms of a *Type 2 explanation.* However, they note that there are limitations in this particular explanatory hypothesis. For example, they ask how the principle of superposition is to be reconciled with the power equation relating dissipating stress to time? The principle of superposition was derivable from the spring and dashpot model, but the power equation was obtained from experiment. It is true that this could have been derived from extending the spring-dashpot model to an infinite number of springs and dashpots related exponentially, but otherwise was not obtainable. The calculus would therefore become exceedingly cumbersome, and unless it was made cumbersome it would not lead to accurate predictions for relating dissipating stress with time.

In an attempt to offer an explanation which will cover the difficulties between linear superposition and the power equation for relating dissipating stress and time, the authors turn to their own solution. This is to derive the relationships from an extension of the Nutting Equation. Now this seems to me to be an attempt at a *Type 1 explanation.* It can be tested in terms of deriving consequences and seeing whether the fit of the data does not significantly differ from the results expected on the basis of the extended Nutting Formula. I take it that this has been done and there is no doubt that as a *Type 1 explanation* the Nutting formula is satisfactory. Moreover, it is more satisfactory because of its having fewer parameters than the equations derivable from even a spring-dashpot model.

The problem then is not whether to accept this *Type 1 explanation,* but to relate it to current and future rheological theory. Graham points out in his paper that this is the point in discussing what he calls the difference between fundamental (already existing) and empirical (construction of *Type 1 explanations*) views. In relating it to present rheological theory one can proceed in two directions.

(*a*) To look for a *Type 2 explanation* of their formula.

(*b*) To look for consequences of their formula which predict an occurrence which would not occur under the spring-dashpot theory. A test of this consequence would be the deciding factor between the two theories. Can this be done?

I take it that the major question in the minds of most critics of the use of the Nutting formula by Graham and Scott Blair is that at the moment there is no *Type 2 explanation* of it.

It is a possibility that no satisfactory *Type 2 explanation* is forthcoming. In this case a search for examples of the application of the equation might be useful. Already it appears to me that a wide range of application has been shown. A similar situation can be seen in a new field of study—General System Theory—discussed by von Bertalanffy,[1] in which a study is made of those systems in which predictions can be made on the basis of isomorphic laws. A study of the characteristics of the complex systems which are covered by such isomorphic laws may give us a clue as to *Type 2 explanations* which might be useful in the various fields. In this way the relationship between power law in rheology and the power equations involved in the phenomena of relative growth in biology [2] might be related. One such model, applicable to Scott Blair's material and biological phenomena of growth would be a model based on the kinetics of enzyme activity, for the biologist believes that the differences in relative growth in different organisms is related to genetic factors affecting rates of enzyme activity in different processes which affect growth.

5 *Conclusion*

We must therefore return to the objections which have been mentioned in the beginning.

(1) That it is inconceivable that all complex phenomena can have a simple explanation. My answer would be that there is no reason why even complex phenomena could not be *described* in terms of a simple generalisation, provided one can describe what one *wants* to describe without introducing all the complexities inherent in the material. This minimum type of description is the basis of all generalisation. If we went to describe the weight of a lump of sugar, or its behaviour as a falling body, then we leave out its colour, its taste, its use etc., and yet can still make meaningful generalisations about it so far as it is to be

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[1] L. von Bertalanffy, 'An Outline of General System Theory', this *Journal*, 1951, I, 134-72

[2] J. H. Huxley, 'Problems of Relative Growth', London, 1932.

described as a falling body. The same sort of difficulty is found in description of human behaviour which is undoubtedly very complex, but advances are made when some of this complexity is shed. Then there is no reason why a *Type 1 explanation* which appeared simple should not be used to derive this simple generalisation. A *Type 2 explanation* will undoubtedly increase the complexity of the explanation, in so far as new concepts are involved in this.

(2) That the phenomenological method that has been followed is mere curve-fitting I do not think in *itself* is a bad thing. If the curves enable more accurate predictions to be made and are more economical of parameters it represents an advance because the curve represents a *Type 1 explanation.* Unless it can be shown that a *Type 2 explanation* cannot, in principle, be suggested from which the *Type 1 explanation* follows, the formula for the curve should be accepted.

(3) That merely to represent results by a formula does not meet the essential need of explanation. This objection rests on an understanding of what is meant by the essential need of explanation. If by this we mean the need for a *Type 2 explanation* then we can attempt to think of the direction of the next step in theory construction which is to *find* such as the *Type 2 explanation.*

(4) That progress can only be made by fundamental studies is quite definitely a false concept. There are two sorts of progress, (*a*) the search for generalisations and *Type 1 explanations,* and (*b*) the search for *Type 2 explanations.* Finding of *better Type 1 explanations* may require the revision of previously existing *Type 2 explanations* as in my clock example.

(5) That a fundamental result must be based upon atomic factors. This is only true so far as one sort of progress is made by searching for *Type 2 explanations.*

Finally, in case we feel that *Type 1 explanations* are unsatisfactory at low levels (low in the sense that they are close to observation records) at *high* levels we *have* to use *Type 1 explanations* until we can see a way of talking about still higher levels. A scientific theory is a calculus and we need not ask first whether the term in it refers to anything. The prime question is whether the formulae expressed at the higher levels are useful in prediction.

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DETERMINISM IN CLASSICAL PHYSICS *

G. F. Dear

In 1951 Professor Popper published two papers[1] in which he argued that classical physics is indeterministic in perhaps no less fundamental a sense than is quantum physics. Popper's argument was in two stages, the first being a purely philosophical preliminary to the second, which was a technical exercise in physics and logic. Firstly, he substituted for ' the metaphysical doctrine of determinism ' the more precise and testable notion of complete, though ' finite ', scientific predictability. Secondly, he tried to show that even in a universe which is ' prima facie deterministic ', that is in one which is completely describable in classical terms, there can exist certain physical systems which are unpredictable.

This paper is divided into two main parts. In the first part (§§1-3) two possible scientific explicata of Determinism are analysed and assessed: predictability is found wanting and scientific lawfulness is shown to be satisfactory and replete. In the second part (§4) Popper's technical thesis is examined and rejected.

Although the two parts of this paper correspond in content to the two stages I have distinguished in Professor Popper's argument, only the second part is directly contrary to what he was concerned to establish. In taking predictability as a testable rendering of the determinist thesis, Popper was not necessarily committed to its adequacy, nor was he necessarily preferring it to scientific lawfulness. Rather, the intention was to attempt to meet the Copenhagen school of quantum theorists on their own ground by adopting without argument a philosophical orientation similar to their own, and thus to make more profitable comparison of classical physics with quantum physics.[2] However, if the argument of the first part of this paper is correct and incomplete predictability is allowed to be not necessarily incompatible

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* Received 10. ii. 60. The author wishes to acknowledge the useful conversations he has had with Dr G. Buchdahl, Professor R. B. Braithwaite, and Professor K. R. Popper while preparing this paper.

[1] K. R. Popper, ' Indeterminism in Quantum Physics and in Classical Physics ', this *Journal*, 1950, 1, 117-133, 173-195

[2] This is of course only my interpretation of what Popper intended.

with determinism, then even if Popper's technical thesis had been valid it would still have had no essential bearing on determinism. Predictability and determinism, it is here maintained, are different things.[1]

I

Before proceeding to explicate determinism let us briefly consider, first, why determinism stands in need of explication, and second, what criteria an adequate explicatum must satisfy.

There are no laboratories, scientists or budgets devoted to determinism. And this is not the result of an oversight or of an unsympathetic D.S.I.R.: it will become clear below that whether the world is or is not deterministic logically cannot be the direct object of scientific enquiry. How then does determinism make contact with the contingent ? Merely as some sort of higher-level scientific programme ? No. Not merely. Though certainly it can be advanced as a programme. But it is worth noting that for a scientific (or meta-scientific) programme to be advanced at all it is necessary that the proposition which affords it rational direction [2] be rationally entertained, or rationally doubted, and hence that there exists some evidence, immediate or theoretical, bearing on it.

What kind of evidence has traditionally been cited in support of a determinist thesis ? Not, roughly speaking, the small or large experimental evidences that bolster or topple specific hypothetical regularities and laws, but rather the fact of the existence of regularities or laws as such, without regard to their particular contents. It is by reflecting on the regularities as regularities that the idea of determinism is born: the idea that clocks, clouds, computers, human beings have something in common other than their patent materiality, vaguely, the lawful way in which they unfold in time. And as it is in the context of the *prima facie* conflict of this idea with the consciousness each man has of being a free person, rather than in the active pursuit of science itself, that the notion of determinism has developed much of its point and meaning the problem is this: to find an empirical thesis which is similar to the original determinist insight in range and content, which is sufficiently exact for the assessment of its truth or falsity to be in principle possible, which is—for preference—simple, and which further, does not in rendering the original empirical, distort it in such a way as to gratuitously resolve the 'Free Will Problem'. Such transit from an unclear, prescientific insight to a decidable scientific hypothesis is not essentially and so necessarily 'psychological' in nature, the result of a random imaginative jitter or of a causal push: it *can* be rational, and the process is then usually referred to as 'explication'.[1]

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[1] Cf. W. B. Gallie, *Free Will and Determinism Yet Again*, The Queens University of Belfast, 1957

[2] E.g. the programme 'find out whether all S and P' is directed either by the hypothesis 'All S are P' or by $\sim$ 'All S are P'.

2

We take as starting point, or explicandum, the neutral:

> By necessity are foreordained all things that were and are and are to come.[2]

Although Democritus intended this scientifically, as a bare proposition, it is of course not specifically scientific, or metaphysical, or, anything too particular. Speaking loosely, three main ideas can be distinguished in the explicandum: necessity, fore-ordination, and the universality of the reference of these two ideas, that is, their predication of *all* events. Whereas all theses that have traditionally been known as 'deterministic' have had the universal reference, the first two ideas need not both belong: theological determinism has usually placed fore-ordination or divine omniscience centrally, the necessity, if it is not rejected, being 'only' the necessity of a divine fiat; metaphysical determinism, as for example that of Spinoza, has dwelt on the necessity and retained the fore-ordination, if it has retained it, merely as a figure of speech.[3]

Roughly speaking, the scientific explicata we shall consider have emphases paralleling those above: predictability is empiricised fore-ordination, and scientific lawfulness maintains the element of necessity in the formal necessity or deducibility of later conditions given earlier ones, for all that only experience can tell which particular theories might in fact be 'laws'.

We shall begin by considering scientific lawfulness, which we shall refer to as 'determinism$_1$' (or d$_1$)[4]. A reasonably precise formulation is the following:[5]

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[1] See R. Carnap, *The Logical Foundations of Probability*, Chicago, 1950, p. 5

[2] Democritus (Plutarch, Strom. 7)

[3] Leibniz, of course, realised both ideas.

[4] For the original explicandum 'Determinism' will be used.

[5] This is a somewhat modified version of that proposed by P. Feyerabend in a discussion in *Observation and Interpretation*, ed. S. Körner, Bristol, 1957, p. 182.

'The world is deterministic if, and only if, the class of all observables can be divided into sub-classes in such a way that for every sub-class there exists a theory which is deterministic with respect to a set of variables consisting of that sub-class plus, possibly, certain "hidden parameters", and which is true', where a deterministic theory is defined provisonally by:

'A theory is deterministic with respect to a set K of variables if, and only if, a conjunction C of statements to the effect that the variables of K have a certain value at the time t_2 can be derived from the theory together with another conjunction of statements, which again contains only the variables of K, and which asserts that those variables had some other value at some other time $t_1 < t_2$.'

Although d_1 is empirical, it is no ordinary scientific hypothesis. It is peculiar in that, being an assertion about the logical or mathematical form of true scientific hypotheses, it bears on the World only via these hypotheses. Its empirical content is that: the theories which truly describe the World are deterministic in form.[1] d_1 is itself descriptive, even though indirectly so.

. What is to count as evidence for or against d_1? Clearly, we can conceive no stronger or more relevant evidence than the deterministic or indeterministic form of the presently accepted scientific theories. Of course, there can be no guarantee that these theories are 'true', in that fresh experimental evidence may always lead to their falsification. However, in so far as they are 'so far forth true' relative to all the evidence to date, a wider evidential base for a d_1 which is to be empirical cannot be conceived, since this base is as wide as science itself.

Determinism$_1$ is presently falsified if there exists one instance of an accepted theory which is not deterministic itself, and for which there are no grounds for asserting it to be reducible[2] to other deterministic theories. For investigators into determinism the accepted theories are to be found in the recognised journals and textbooks. Assessment of the truth of those theories, of the extent to which 'hidden parameters' are acceptable variables in a theory, and of the plausibility of attributing a distribution of errors about a predicted value of a variable to determinate, though perhaps uncontrollable, variations in the boundary conditions, are matters for scientists and not for determinists$_1$. At present the evidence is against d_1: quantum mechanics plus the Born probability interpretation have still to be reduced to a more basic deterministic theory.

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[1] Cf. L. Wittgenstein, *Tractatus Logico-Philosophicus*, London, 1922, **6**, 32.

[2] E.g. in the sense of 'reduction' introduced by Kemeny and Oppenheim: 'On Reduction', *Phil. Studies*, 1956, **7**, 6–19

In passing, it is worth noting briefly why determinism$_1$, although an 'all-and-some' proposition possessing both universal and existential quantification,[1] evades the 'metaphysical' label recently attached by J. W. N. Watkins to the superficially similar Law of Universal Causation.[2] Whereas it is, I suppose, still just possible for an empirically-minded metaphysician to insist that, all appearances notwithstanding, a cause will always be found for every effect if one looks long enough—though how and where to 'look' is of course not specified—yet d_1 cannot be so easily compromised. Theories are human artefacts. Their existence is not in doubt. Neither is their deterministic or indeterministic form. So that one need look only as far as the libraries.

For purely mathematical reasons we shall now loosen somewhat our definition of a deterministic theory (see p. 12). There are two difficulties: firstly, for the vast majority of problems the equations of mathematical physics can only be solved approximately,[3] either by 'perturbing' a purely analytic solution of a simpler problem, or by solving numerically from scratch; secondly, if the time interval $t_2 - t_1$ is allowed to become infinite, mathematical singularities and paradoxes multiply rapidly. These difficulties are avoided by requiring of our theories not exact temporally-unbounded solutions, but rather solutions for a finite time interval obtained after a finite number of deductive steps: given precise values of the variables for the earlier time t_1, it must be possible to derive, for a later time t_2 (where $t_2 - t_1$ is finite), values of the variables which are as close to the 'true' values as we please. Or, we require of a deterministic theory convergent solutions over a finite time interval.

But what if, for certain special problems for example, the solutions are not convergent?

Such difficulties are emphatically not for determinists$_1$, or indeed for pure mathematicans (once the non-convergence is recognised), but for scientists. There are two alternatives: either there is a proof of non-convergence, or there is not such a proof. If there is a proof, then the scientist must decide whether the non-convergence, or perhaps the complete lack of any sort of solution at all, is to be attributed to inadmissible idealisations in setting up the problem for the theory,[1] or whether rather, having a problem which is just not effectively covered by the theory (since it gives no solution), he ought not to look for a theory which *does* cover the problem. For the second alternative: if there isn't a proof of non-convergence and it is uncertain whether a solution exists, then either the scientist will look for a better theory, or he will 'rationally intuit' that a mathematical discovery will one day give him a solution. And as he knows the problems and has all the evidence, is it for us to quarrel with his judgment ?

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[1] E.g. Roughly, d_1 can be written as 'All observation statements are subject to some deterministic theory'.

[2] See e.g. 'Confirmable and Influential Metaphysics', *Mind*, 1958, 67, 344.

[3] Even when probabilities are not explicitly involved.

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As an alternative explicatum, favoured by some, we consider now predictability, which will be referred to as determinism$_2$ (or d$_2$) and which initially is simply: 'All events are predictable.' [2]

Determinism$_2$ is sometimes advanced as though it were too obviously an adequate scientific explicatum of Determinism to require justification, especially if Determinism itself is conceived in theological terms, in terms of predestination or of omniscience. More often however, it is put forward either as being in some sense an analysis of what d$_1$ amounts to when direct reference to the—on this view—purely mediatory theories is avoided, or as being an empiricised version of a position which is otherwise 'metaphysical'.

That d$_1$ is content-full has been argued above. We shall now show that d$_2$ is significantly distinguishable from d$_1$, and so is not acceptable as an 'analysis' of it, and shall briefly explore d$_2$'s inadequacy as an explicatum of Determinism.

At first sight, d$_2$ appears to possess an epistemological immediacy and a simplicity so conspicuously lacking, the reader may feel, in d$_1$. For is not 'All events are predictable' of the familiar form 'All α are β' and such that the instances 'α_1 is β', etc., are virtually observation statements ? Cannot it be very simply tested by placing a good scientist in any arbitrary observational field and noting his performance:

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[1] And this, for example, is the case for three-body collisions in Newtonian mechanics: punctiform masses do not exist.

[2] Cf. the more exact 'finite' version of Professor Popper given in section 4 below, The analysis and criticism of d$_2$ which follows applies equally to it.

if he makes successful signals then *that* particular kind of α is β, that is, it is predictable ? But d_2 is not at all simple.

An invariable first step in developing d_2 must be to understand by 'predictable': predictable by means of scientific theories. This is necessary if revolver-compulsion and/or successful Old Moore universes are to be excluded.[1] This first specification of the meaning of 'predictable' removes at once the epistemic simplicity of d_2: d_2 is now a proposition operating on two different levels. On its own level—which we shall call the meta-level—it asserts the universal possibility of an 'intelligent' object, e.g. a scientist, successfully predicting any specified event. And the 'predicting' here is an activity performed in time and space by a contingent being, the success of a prediction being inferred from the intelligent object's behaviour. But this object is also a subject. At the lower level—lower because it is circumscribed, dependent on the experimental and other conditions obtaining at the meta-level—the subject or scientist performs logical operations within the appropriate scientific theories, and assesses the truth-value of his inferences against his observations.[2]

The fundamental objection to d_2 is that scientific theories play a part at both of these levels, and that because of the 'subjectivity' at the meta-level an infinite regress is generated. Any remotely acceptable d_2 is saturated through and through with theory at its own level, as well as at the lower level. Only thus can it be given the necessary universality and the anthropocentricity or 'subjectivity' inherent in d_2 be at least superficially mitigated. To secure this, there is always added to 'All events are predictable' the sweeping qualification 'in principle'. Any appeal to this qualification is an appeal to theory.

Firstly, let us suppose that our intelligent being is *very* intelligent, his grasp of scientific theory unrivalled, his observational acuity a by-word: what would we need to add to a finite number of successes to secure 'all α are predictable by a perfect scientist' ? Spatial and temporal universality. If space-time is accredited—by the relevant *theories*—with homogeneity the inference is not very difficult. It corresponds to the early extension of the verificationist meaning criterion to the past, and to distant points (e.g. Mars) which are unattainable for merely ' practical ' reasons. On similar grounds it is recognised that *all* the events occurring during any one short period of time will never, unless the universe changes radically, be predictable, because the number of events will always be larger than the number of perfect scientists. In short, all those events are admitted which could be predicted were a scientist or an observer-calculator to be present, and for which the presence of such an observer-calculator at a spatio-temporal point appropriate to observation and prediction of the event is not contrary to accepted scientific law.

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[1] It is worth noting here that since 'the scientific theories' are not specified, d_2 no less than d_1, is an 'All-and-some' proposition and thus needs rescuing, just as much as does d_1, from the 'metaphysical' label.

[2] The acceptability of 'errors' is assessed at this level also.

But no scientist is perfect. Scientists sometimes make logical or mathematical blunders, they are not always competent, they are subject to hallucinations, wishful thinking, fatigue. And if a scientist persistently gets results which differ from the general opinion it will be, and is, put down to bias or crankiness or ego-involvement. The worth of a scientist's conclusions is measured against his known ' properties ' and any light thrown on these by the vogue psychological theories. Any peculiarities of the observer-calculator are thus granted to be very relevant in any consideration of d_2.

But although account can be taken in d_2 of the peculiarities or ' accidents ' of the observer-calculators, ' essential ' properties must remain unacknowledged, *even though they be recognised.* For d_2 cannot be further ameliorated without depriving the notion of ' predictability ' of its activity-like characteristics and thus of that component of its content that differentiates it from d_1, for which ' predicting ' will of course signify only a sequence of logical operations given certain boundary conditions. In particular it cannot be stretched to admit one or more classes of events to be unpredictable by any scientist whose perfection leaves him still human: even though many members of a class of events may not in fact be predictable, yet a member of the class must in principle be predictable. D_2 is presently refuted[1] if a scientist[2] operating under ideal conditions and making the maximum number of relevant and compatible observations, finds himself unable to predict the outcome of an experiment, and this unsuccess is repeatable. While the ' in principle ' qualification allows the scientist to account for his present failure and promise a future success, by pointing *outwards* at contingently unsatisfactory features in his apparatus or in his observations or in his calculations, it does not allow that he invoke properties essential to him in his capacity as scientist or man to explain away and excuse perpetual and perhaps inevitable failure. Although therefore at the lower level, the scientist operates with the rich language of scientific theory—which may be as hypothetical as current acceptance decrees, and which, if the theories form a complete lawlike base for the world will be as applicable to the scientist as object, as they are to the events he studies—yet at the level of d_2, instead of using this same rich language to explain perhaps why not only he, but indeed *any* scientist would be unable to perform certain kinds of predictions, a wholly arbitrary boundary, scientifically-speaking, between accidental peculiarities and essential properties is unjustifiably drawn. Moreover, the first step in a confusing logico-empirico regress in which each step must, on account of the empirical component in a prediction—of predicting as activity—be shorter than the one that precedes it, and in which Gödelian self-references and (predictions)^n leer frightfully from their filigrees, is pointlessly generated.[1]

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[1] i.e. relative to the presently accepted theories.

[2] Or a team of scientists.

As an attempted scientific explicatum of Determinism, d_2 is radically misconceived. Its assessment and acceptability cannot but rest, as does that of d_1, on the presently accepted scientific theories. Else it is not scientific.

Yet whereas the evidence for d_1 is simply and non-self-referentially the syntactic form of those theories, that for d_2 is rather the complicated inter-relationship of *all* of those theories with those among them which describe the operations of contingent knowers and calculators. Such involution is foreign to Determinism: Determinism is an assertion about all events, all things, all processes, knowledge and prediction complexes no more, and no less, than others less differentiated. The possibility of the occurrence of certain behaviour patterns, and in particular of successful prediction characteristics, in some of the organisms or systems within and interacting with the World is of no central relevance. That a difference of physical levels sufficient to allow prediction-language to operate may not always be possible, is

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[1] Of course, not all of the predictions relevant to establishing d_2 will, even as activities, depend significantly on the essential characteristics of the scientist. But even though we do not recognise the prediction singularities which Popper has claimed to establish for mechanical predictors (see § 4), yet there are at least two analogous types of ' complementarity ', in psychoanalysis and in quantum mechanics, in which we would not wish to exclude *a priori* the existence of underlying determinist substrata, even though complete finite predictability may not ever be possible. In any universe in which complementarity (i.e. strong knower-known interactions) was known to occur, d_2 would be distinguishable from d_1.

a priori no more interesting a fact than, say, the impossibility of perfect heat insulation.

What of the 'necessity' and 'foreordination' of the explicandum? Whereas in d_1 the former is represented as succesfully as is possible in these days of non-nomic necessity by the necessity 'implicit' in the deterministic theories, in d_2 even this very much weakened necessity is tainted by 'transmission' through the merely contingent properties of the predictor. 'Foreordination' is indeed explicitly empiricised in d_2: but attempting to encapsulate the omniscience of divine intuition in a law-determined law-intentional predictor only clouds the clarity of the laws, and in any case does no justice to the intention of Determinism.

4

In this section we attempt to show why Popper's technical thesis is false. This thesis asserts that the proposition:

> 'For any specified finite prediction task, it is physically possible to construct a predictor[1] capable of carrying out this task '[2]

is incompatible with classical physics.

By a 'specified finite prediction task' is meant:

> the task of predicting, with some chosen and specified degree of precision, some event—i.e. the position and or the velocity of its particles—occurring in a finite mechanical system, sufficiently isolated from outside interference, at a certain chosen future instant of time.[2]

A summary of the argument runs as follows:

> Consider a mechanical system A, not containing a predictor, and a predictor B attempting to predict A. This it can do only if (1) B can calculate the results of its interference with A or if (2) B interferes sufficiently weakly with A.
>
> As to (1), B can assess the way in which it interferes with A either by studying its interfering parts B′ and their interaction with A—which means that it has to study the system A + B′ instead of A—or on the basis of predictions about itself. The first of these two alternatives does not help for in order to obtain the necessary information about A + B′ the whole problem arises again. The second alternative does not help for it will be shown in the next section '8' that a predictor cannot have such information about itself. Thus we must consider (2).[3]

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[1] For a description of a predictor see Popper, op. cit., 123

[2] Popper, op. cit., 124

[3] Popper, op. cit., 128

Popper then shows (p. 130 following) that if A *does* consist of a predictor employing the same physical principles as B (or if A and B are of the same 'physical order') (2) is not the case, so that when in '8' self-prediction is shown to be impossible the thesis that, 'if there are classical mechanical predictors . . . then there must exist unpredictable predictors' (p. 133), is apparently established.

We shall first state briefly why we find Popper's argument mistaken, and then describe how a predictor able to perform any specified finite prediction task might in fact be constructed. General criticisms of the standpoint adopted throughout the two papers are that:

(i) The physical construction of the predictors is never made really explicit.

(ii) It is assumed that 'interference' is almost a tertium quid coming between predictor and the system to be predicted, requiring apparently separate 'calculation', ((1) above, and hence the necessity of self-prediction), and indeed study as a separate physical object (note the argument following (1) and in particular the assertion that 'the whole problem arises again').

(iii) 'Knowledge', in particular 'self-knowledge' and 'self-information', are given a certain hypostatised subsistence inconsistent with the purely scientific and single-level viewpoint which must be adopted in considering the physical possibility or impossibility of the occurrence of a particular type of sentence token.

This last brings us to the grounds on which we must reject Popper's demonstration, which is that: the sense in which self-prediction is shown to be impossible is not the sense in which self-prediction is necessary in order to allow for the effects of interference. If we distinguish self-prediction qua computer, from self-prediction qua interaction, then since only the latter is relevant in calculating the effect of interference of the predictor, B, with the system to be predicted, A, Popper's conclusion does not follow, even if we grant that self-prediction qua computer is impossible (and '8' is concerned wholly with this). Formally then, if the distinction be allowed Popper's argument is four termed and hence invalid.

But is such a clear distinction between 'computer' and 'interactor' always possible?

It must be understood that in order to disprove Popper's thesis it is only the *possibility* of always being able to construct, for a given specified task, a predictor in which the distinction is embodied and which can therefore perform the task, that we need to establish. It will of course always be possible to design a 'predictor' which is so confused and inefficient in its working as to be quite incapable of predicting anything, but from the possibility of such bad design nothing follows. However, we shall in our discussion below of the construction of an efficient predictor indicate why the distinction is not only possible but indeed necessary if the property of being a 'predictor' is to be conferred on an otherwise lustreless physical object.

A predictor is a device for making predictions about the future which the designer of the predictor would adjudge to be rational. In order for this to be the case the predictions must be derivable from a theory (together with certain boundary conditions), which is, if the predictions are to be definite and precise, a deterministic theory. If the system A, whose state at some future time t_2 is to be predicted, has f degrees of freedom it is necessary that B, the predictor, should be able to perform two distinct sets of operations:

(i) the measurement of $2f$ independent parameters specifying the state of A (if the equations of motion are second order). The values of these parameters are the initial conditions at time t_1.

(ii) 'knowing' the f equations governing A's motion it must solve them for the time t_2, before t_2 has arrived.

For (i) B requires a part B′, the interactor, interacting with A through at least f measuring arms (These 'arms' may be mechanical tentacles, thermometers, beams of light, or any other mode of interaction which yields a pointer reading uniquely determined by the conditions obtaining at the place of interaction). If the effect of B′ on A is small relative to the degree of precision required in the predictor it can be left in continuous contact with A without ill effect. Most classical measurements are of this nature and, giving rise to no prediction difficulties, correspond to what Popper has called 'electromagnetic miracles'. For (ii) B requires a part B″, the computer.

We shall consider only the case in which the interaction between B′ and A is strong, and our predictor will operate in the following manner.

(*a*) At time t_1 B′ is placed in contact with A for a time Δt_a seconds after which time it is withdrawn from contact in a determinate manner.

(*b*) Δt_b seconds after withdrawal from A, B′ communicates to B″, the computer, $2f$ pointer readings virtually instantaneously and is then disconnected. In this communication no *physically* important interaction occurs; any attenuation, etc., in the pointer readings of B′ during the communication can be allowed for in B″'s interpretation of these readings, provided always determinateness, i.e. uniqueness, obtains.

(*c*) B″ is now successively programmed to have a logical structure isomorphic with a method of solution of: the equations governing the motion of B′ alone (solved retrodictively for a period of time Δt_b, from $t_1 + \Delta t_a + \Delta t_b$ to $t_1 + \Delta t_a$); the equations relating the $2f$ independent parameters of B′ to the $2f$ independent parameters of A (those will be determined by the structure of the composite system A + B′); and finally, the equations of motion of A alone (solved predictively for the period $t_1 + \Delta t_a$ to t_2). Suppose that the whole calculation takes Δt_c seconds. The resultant prediction is produced, for example, in the form of a punched tape.

We must consider several points in this account in greater detail.

All measurement involves a physical interaction and a consequent disturbance of the measured system. If A is a very delicate system it may be the case that any B′ we can design interacts so strongly with A during the period Δt_a of contact as to distort seriously or even to transform the future of A. However this has no bearing whatsoever on whether we can predict that future. So long as the interesting singularities of the Oedipus effect[1] are avoided, i.e. so long as the prediction is a logical deduction rather than a physical interactant and causal determinant, the fact that knowledge or information can only be obtained at the price of interference is a fact which is no harder than most facts.

In order that a prediction should be a scientific prediction (i.e. a prediction with rational backing) and not just a prophecy, it is necessary that it be logically deducible from certain other propositions which make some sort of claim to some sort of truth. The form of a deterministic theory requires that certain initial conditions be given once and for all, and then that certain mathematical equations be solved in a logical manner. All well-designed predictors applying determinist theories must accordingly be constructed so that B″ the computer is (during the computation) 'logically isolated' from A and B′ once the initial communication of information has taken place. By 'logical isolation' is meant: the physical events in B″ must, in so far as they possess logical or linguistic significance, be physically independent of any events occurring in A and B′. For if B″ is to be a physical embodiment of certain logical and mathematical operations the only physical events occurring in B″ and affecting the final prediction must be events possessing a propositional interpretation. Hence the distinction between predictor qua computer and predictor qua interactor is not only possible in fact, as we shall see almost at once, but is logically necessary.

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[1] See Popper, op. cit., 188. And this effect *must* be avoided (or be negligible) in any scientific prediction which is to be acted upon by rational men (who will not be unconscious of this fact too).

The simplest way of achieving ' logical isolation ' is to make B′ and B″ physically quite distinct as suggested above. In certain cases (i.e. for certain A's), this physical isolation may not be possible, but even then the interference must be present only ' as a perturbation ', and problems of self-reference and feedback circumvented if the logical character of the prediction is to be preserved.

But even granting that such ' logical isolation ' is logically necessary is it always—for all specified prediction tasks—physically possible ?

Since it would appear to be necessary for all predictors to be amplifiers[1], and since most modern computers are digital, the most convenient way of making contact between B′ and B″ would be by electronic means. This would have two advantages. Firstly, if all B″'s readings are expressed as fluctuating potentials on the grid of a valve the reaction of B″ on B′ can be made very small (so that the ' attenuation, etc.' mentioned in (*b*) above would be unimportant because electronically an ' electromagnetic miracle ', or the one-way passage of information, is always possible; note, however, that a finite reaction would not affect the argument). Secondly ' logical isolation ' can be very simply achieved by ' biasing off ' the input valves after the initial communication of information. Although electronic devices would appear to be applicable to all classical prediction tasks (so that the question above is answered affirmatively) the possible use of electronics is not essential to this argument. Temporary mechanical contact between B″ and a B′ disconnected from A is an alternative (e.g. for analogue computers).

This concludes the discussion of the construction of an efficient predictor. Basically it has centred round the fact that any definite or uniquely determinate interaction with a physical system is potentially interpretable as a measurement of that system, and that if the observed system possesses f degrees of freedom any $2f$ independent pointer indications will, no matter how processed, enable its future to be predicted provided that they somehow filter through to a logically isolated computer in a determinate manner.

Let us now suppose that the system A is itself a predictor. Can the predictor B predict A's future state ?

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[1] Popper, op. cit., 129

If A is itself predicting a system X the problem is simple. If B happens to be a more efficient predictor than A it can, by looking at A's amplified input from X (and this need not disturb A or X) and operating a programme based on A's logical structure when it is reacting to X, predict A's future behaviour and in particular A's prediction of X. But if B is slower in its computations than is A it will not be able to predict A's future prediction for the very good physical reason that

$$\Delta t_a + \Delta t_b + \Delta t_c > t_2 - t_1 \text{ (See } (a), (b), (c) \text{ above)}$$

More interesting is the possibility of prediction when A is studying B and B is studying A, and A and B are of the same physical order (i.e. the interaction is strong). This is the problem with which Popper is initially concerned.

If A is to qualify as one of Popper's 'finite prediction tasks' there are two and only two possibilities.

(*a*) B is required to predict only A′, the interactor. Both the construction of A′ and the rules by which the feelers of A′ are instructed (by A″) to search out its surrounds must be programmed into B, which can then perform its task. For this case it would be possible both for B to predict A′ and A″ to predict B′. Since the interaction is strong A′ and B′ will exert a strong influence on one another's futures.

(*b*) B is required to predict the future not only of A′ but of A″ too. In order to do this all of A's programmes, together with instructions about their use must be known by B, and further, B″ must be a faster computer than A″.[1] It is logically impossible to have B totally predicting A, and A totally predicting B without a solution of the problems of self-reference,[2] but this has no bearing on B's ability to predict any *specified* physical system A, and so does not concern us.

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[1] How such knowledge is obtained by B is really irrelevant to our problem in which we are concerned only with *specified* A's. The knowledge could be programmed into B by the makers, or obtained by B studying A prior to the actual prediction task. During this prior examination it would be necessary that: (i) A be incapacitated in its function (if it has one) of self-programming computer (B, having cosmic aspirators, might be programmed to remove, on occasion, the power supply to any A); or (ii) B was able to arrive at a satisfactory hypothesis explaining A and its self-programming potential *in toto*.

[2] The problem of self-reference as it occurs in Popper (loc. cit.) is: given that S is the case is it possible to construct a language L such that 'S' describes S + "S" where "S" is the sentence token for 'S'. Given a *finite* number of possible states ($S_1 S_2 \ldots Sn$) there seems to be no reason why a suitable L should not be so engineered.

We therefore find that the only obvious limitations imposed by virtue of the physical nature of predictive activities on the prediction of A by B are due to:

(i) the finite operating speed of the computer and of measurement (requiring $t_2 - t_1 > \Delta t_a + \Delta t_b + \Delta t_c$),

(ii) A not being sufficiently large to interact with some B′,

(iii) the physical impossibility of B learning of the construction of A.

Neither (ii) nor (iii) are the case if A is itself a predictor and is a finite prediction task, and (i) is a physical requirement devoid of any logical significance.

We conclude that predictors are in principle predictable, and so that there is therefore no logical reason why in a determinist universe any system should not be predictable.

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A CAUSAL CALCULUS (I) *

I. J. Good

1 *Introduction*

This paper contains a suggested quantitative explication of probabilistic causality in terms of physical probability.[1] The main result is to show that, starting from very reasonable desiderata, there is a unique meaning, up to a continuous increasing transformation, that can be attached to 'the tendency of one event to cause another one'. A reasonable explicatum will also be suggested for the degree to which one event caused another one. It may be possible to find other reasonable explicata for tendency to cause, but, if so, the assumptions made here will have to be changed.

I believe that the first clear-cut application in science will be to the foundations of statistics, such as to an improved understanding of the function of randomisation, but I am content for the present to regard the work as contributing to the philosophy of science, and especially to what may be called the 'mathematics of philosophy'. Light may also be shed on problems of allocating blame and credit. I hope to consider applications to statistics on another occasion.[2]

In a previous note[3] I have tried to give an interpretation of 'an event F caused another event E' without making reference to time. It was presumably clear from the last three paragraphs, which were added in

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[1] Compare : (i) Hans Reichenbach, 'Die Kausalstruktur der Welt und der Unterschied von Vergangenheit und Zukunft', *Ber. d. Bayer. Akad. d. Wissensch., math.-nat. Abt.*, 1925, pp. 133-175; or Chapter 3 of his book *Modern Philosophy of Science*, London and New York, 1959; (ii) Norbert Wiener, 'The theory of prediction', in *Modern Mathematics for the Engineer* (ed. by E. F. Beckenbach), New York, 1956, pp. 165-190. The present work goes further than Reichenbach's in offering a definite explicatum. It bears little resemblance to Wiener's work, which is mathematically much more advanced.

[2] The present paper owes much to correspondence and discussion with Mr E. M. L. Beale, Professor Bruno de Finetti, Professor K. R. Popper, Professor L. J. Savage, Mr Christopher Scott, and especially to Dr Oliver Penrose. The Referees and Editor have also been helpful.

[3] This *Journal*, 1959, 9, 307-310

proof,[1] that I was not satisfied with my attempt.[2] I shall describe the note as the 'previous paper' but it will not be necessary for the reader to refer back.

The present paper is more ambitious in that it is quantitative, but less so in that it assumes, at least at first, that F is earlier than E. (It may be possible to interpret the explicatum more generally.) As in the previous paper I shall take for granted the notion of physical (= material) probability. In order to avoid misunderstanding I must mention my opinion that in so far as physical probability can be measured it can be done only in terms of subjective probability, but this opinion will not affect the arguments below. Likewise the notion of an 'event' will be taken for granted. In some earlier drafts I included material dealing with the meanings of 'event', 'probability', and 'definition', and with the modifications of the analysis required in order to cope with the possibility that the future may affect the past. I have omitted this material here for the sake of brevity.

2 *Notation and General Outline*

Propositions and events will be understood in a very general sense, and will be denoted by the symbols E, F, G, H, and U. These will be combined by means of the logical connectives '.' meaning 'and', '$-$' meaning 'not', and '**v**' meaning 'or'. A vertical stroke, '|', will mean 'given', as in the expression $P(E \mid H)$, the probability of E given H. Similarly $O(E \mid H)$ will mean that the odds of E given H, i.e. $P(E \mid H)/P(\bar{E} \mid H)$. Sometimes some or all of what is 'given' is omitted from the notation for the sake of brevity. A colon will be used to mean 'provided by' or 'by' or 'from', as in

$$I(E : F \mid G) = \log(P(E \mid F \,.\, G)/P(E \mid G)),$$

which can be read from left to right as the amount of information concerning E provided by F given G. Another example of the colon notation is

$$\begin{aligned} W(H : E \mid G) &= \log(P(E \mid H \,.\, G)/P(E \mid \bar{H} \,.\, G)) \\ &= \log(O(H \mid E \,.\, G)/O(H \mid G)) \\ &= I(H : E \mid G) - I(\bar{H} : E \mid G), \end{aligned}$$

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[1] The words 'added in proof' were omitted in error, and the effect was peculiar.

[2] I find that Reichenbach made a similar error when defining the expression 'causal relevance'. See Appendix I.

the 'weight of evidence concerning H provided by E given G'.[1]

The general plan of the paper is to suggest explicata for:

(i) Q(E : F), or Q for short, the 'causal support for E provided by F, or the tendency of F to cause E'. The explicatum that the argument forces upon us is the weight of evidence against F if E does not happen, $W(\bar{F} : \bar{E})$, or more explicitly, $W(\bar{F} : \bar{E} \mid U . H)$, where U and H are defined below. In order to formulate enough desiderata it is necessary to introduce some auxiliary notions.

(ii) The strength of a causal chain joining F to E.

(iii) The strength of a causal net joining F to E. (Causal chains and nets will be defined in Sections 8 and 11.)

(iv) $\chi(E : F)$, or χ for short, the contribution to the causation of E provided by F, i.e. the degree to which F caused E. This will be defined as the strength of a causal net joining F to E, when the details of the net are completely filled in, so that there are no relevant events omitted. (I avoid the use of the letter C for either Q or χ, because it has been used to mean corroboration.[2]) An example is given in Appendix II to show that Q and χ cannot be identified.

It would not be appropriate to define χ as the limit of strengths of more and more detailed nets; for, if space and time are continuous, the limiting operation could be done in a biased manner so as to get entirely the wrong result; like a lawyer making a case by special selection of the evidence. We must have the whole truth in order to define χ in principle. (Compare the first Appendix.) If, however, the events fill the relevant parts of space and time, and we let the events become smaller and smaller, then the limit should be unique.

In practical uses of the notion of causality, judgments of approximate irrelevance are always made in order to reduce the complication of the causal net.

It is possible to draw an analogy between a causal net and an electrical resistance network, with a resistance in each link. In this analogy it is necessary to imagine a rectifier placed in each link in order to prevent a flow of causal influence backward in time. It is then tempting to define the degree of causality between the input and output of the causal net as the effective resistance of the corresponding causal network. This analogy suggests that the causal resistances should be defined in such a manner that they are additive for chains in 'series', and such that their reciprocals are additive for chains in 'parallel'. It turns out that the analogy cannot be pressed as far as this, but it is one of the themes of the paper.

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[1] See, for example, 'Weight of Evidence, Corroboration, Explanatory Power, Information, and the Utility of Experiments', *J. Roy. Stat. Soc., ser. B*, 1960, 22, 319-331, where there are further references.

[2] Karl R. Popper, *The Logic of Scientific Discovery*, London, 1959, pp. 379-418, mostly published in this *Journal*, 1954, 1957, and 1958. See also Note 1 above.

The main part of the paper consists of a list of assumptions, and deductions from them, leading up to the above explicatum for Q. Afterwards a general explicatum will be suggested for χ, but this will not be deduced in the same formal manner. It is fairly convincingly unique for causal nets of the 'series-parallel' type, and has a certain cogency in the general case.

3 *Small Events*

Until near the end of the paper all events will be assumed to occupy small volumes of space (more precisely: have small diameters) and occupy small epochs of time. For the most part 'space' could be interpreted in a more general sense than as ordinary three-dimensional space; for example, it could be phase space or Hilbert space. On the other hand time will be assumed to be well-ordered and one-dimensional. There must be some sort of metric in both space and time, in order that smallness and contiguity should have a meaning. If the metrics of space and time have been mixed up, as in the theory of relativity, then they will be assumed to be unmixed by the use of a fixed frame of reference. (Dr O. Penrose has pointed out that the present work is consistent with the theory of relativity provided that causal influence does not travel faster than light.)

4 *Causal Support, or Tendency to Cause*

Let H denote all true laws of nature, whether known or unknown, and let U denote the 'essential physical circumstances' just before F started. When we talk about 'essential physical circumstances' we imply that the exact state has a probability distribution. An equivalent description is to say that a system is one of an 'ensemble'. (I must admit that there is more than meets the eye in this description, since in quantum mechanics the word 'state' can be given at least eight interpretations, seven of which may be relevant here. See Appendix 3.)

In order to arrive at explicata for Q and χ I have found it necessary to discuss them in an interconnected manner ; i.e. there do not appear to be enough desiderata for Q, considered by itself, to circumscribe its possible explicata to a satisfactory extent.

In the present section the ground will be cleared by discussing what Q and χ depend upon. It is convenient to think of this dependence in terms of notation, which seems to bring out the main points better than a purely verbal discussion. For example, the symbols Q and χ are abbreviations for $Q(E : F)$, and $\chi\,(E : F)$, and these expressions are themselves abbreviations for $Q(E : F \mid U . H)$ and $\chi(E : F \mid U . H)$. To take U and H for granted, and omit them from the notation, is parallel to linguistic usage. If we say that it is bad for eggs to throw them in the air, we take it for granted that there is a law of gravitation, and that there is a large gravitational body nearby.

Events later than F and earlier than E may be relevant to χ but not to Q. Accordingly I shall assume that $Q(E : F)$ depends only on $P(E \mid F)$, $P(E \mid \bar{F})$, $P(E)$, and $P(F)$. It is natural to define $Q(E : F \mid G)$ as the same function of these four probabilities, but made conditional on G.

Even $Q(E : F \mid U . H)$ is an incomplete notation. If the subjective element is to be removed from the expression ' F caused E ', then it must be expanded to ' F, as against $\bar{F}_D$, caused E rather than E′ ', where the suffix, D, to $\bar{F}$ (the negation of F), represents a complete specification of the relative probabilities of the mutually exclusive events whose disjunction is $\bar{F}$. (D represents a probability distribution.) We could use a notation like

$$Q(E/E' : F/\bar{F}_D \mid U . H)$$

or

$$Q(E : F/\bar{F}_D \mid U . H . (E \vee E')),$$

the degree of causation of E rather than E′ by F rather than $\bar{F}_D$.

The failure to recognise all the variables on which tendency to cause is based was for me one of the stumbling blocks in capturing the notion of probabilistic causality, if indeed I have fully succeeded even now.

It should be held in mind that $E \vee E'$ is regarded as taken for granted in the four probabilities on which Q is assumed to depend, when we are concerned with the causation of E rather than E′. When we take $E \vee E'$ for granted we may write $\bar{E}$ instead of E′.

5 *Assumptions and Deductions Leading to the Explicatum for Q*

In order to make my assumptions clear I shall list them in the form of axioms, A1, A2, . . . ; and the deductions from them will be called theorems T1, T2, . . ., for ease of reference. On a first quick reading the justifications and proofs should *quite definitely* be skipped, but I have not postponed them to a later section. (I did so in an earlier draft, but the cross-referencing made the paper more difficult to read.) The justifications of the most easily acceptable axioms, and the proofs of the easily proved theorems will be omitted.

Let $P(F) = x$, $P(E \mid F) = p$, $P(E \mid \bar{F}) = q$, $P(E) = r$, so that

$$r = xp + (1 - x)q,\ x = (r - q)/(p - q).$$

Unless $p = q$ (in which case $r = p = q$), x is a function of p, q, and r. Therefore by an assumption of the previous section we have :

A1. $Q(E : F \mid G)$ *is a function of* p, q, r, *unless perhaps* $p = q = r$. *We call this function* $Q(p, q, r)$ *so that the symbol* Q *has two slightly different meanings. The symbol* G *will usually be taken for granted and omitted.*

A2. Q *is a real number or* ∞ *or* $-\infty$; *but it may be indeterminate for special values of* p, q, *and* r, *such as when two of them are equal or one of them is* 0 *or* 1. (It seems sensible to look for a scalar explicatum rather than a ‘ vector ’.)

The next two axioms deal with monotonicity and continuity.

A3. (i) Q *increases with* p *if* q *and* r *are held constant.*
(ii) Q *decreases when* q *increases if* p *and* r *are held constant.*

A4. Q *is continuous except when it becomes infinite or indeterminate, if there are such points.*

A5. *If* $P(F) \neq 1$, *meaning, as usual,* $P(F \mid U . H) \neq 1$, *then* Q *has the same sign as* $p - r$, *and therefore also the same sign as* $p - q$, *and as* $r - q$; *and if these expressions vanish we may say that* F *has no tendency to cause* E, *and we put* $Q = 0$. (This axiom removes a gloss from A1.)

A6. *Any causal net joining* F *to* E, *as defined below in Section* 11, *has a causal strength,* S, *and a causal resistance,* R. *These are positive numbers, except that if* $p = q = r$, *or if* p *or* q *is* 0 *or* 1, *we may get zero or infinite resistance or strength.* (An important part of the definition of a causal net is that it consists only of events that actually occurred or will have occurred.)

A7. *There is a functional relationship between* R *and* S, $S = f(R)$, $R = g(S)$, *where* f *and* g *are absolute functions inverse to one another.*

A8. *f and g are continuous decreasing functions.*

A9. $\chi(E : F)$ *is the strength of the complete causal net joining* F *to* E. *More precisely, it is the limit, as the sizes of the events tend uniformly to zero, of the strengths of nets; where each net of the sequence joins* F *to* E, *consists of a finite number of events, and omits no events temporarily between* F *and* E. It is not claimed that this axiom is formulated with complete rigour, but it is used in only a weak form for the explication of Q (in the proof of T14). It is introduced at this early stage in order to supplement the outline in Section 2. (If we assume that the degree to which F caused E has an objective meaning, with a precise numerical value, we are committed to the idea that there is a complete world, uninfluenced from outside. Outside influence could be allowed for by assuming that the numerical values are not absolutely precise.)

A10. *The strength of the causal net consisting of* F *and* E *alone is equal to* $Q(E : F)$ *when this is positive, and is otherwise zero.* (We shall clearly get nowhere unless we assume some relationship betwen Q and S, and A10 is the simplest reasonable relationship that could be assumed.)

The strength and resistance of a net, $\mathfrak{n}$, joining F to E, are denoted by $S(E : F \mid \mathfrak{n})$ and $R(E : F \mid \mathfrak{n})$.

A11. *Let* $\mathfrak{n}$ *be a* 'chain', $F = F_0 \rightarrow F_1 \rightarrow \ldots \rightarrow F_n = E$. *Then* $S(E : F \mid \mathfrak{n})$ *is some function,* $\phi(s_0, s_1, \ldots, s_{n-1})$, *of the strengths,* $s_0, s_1, \ldots, s_{n-1}$, *of the links.* Here

$$s_i = S(F_{i+1} : F_i \mid U_i \,.\, H),$$

where U_i represents the essential physical circumstances just before F_i began. (Causal chains are formally defined in Section 8.)

T1. $\phi(s) = s$. (Proof from A10 and A11.)

A12. ϕ *is a symmetrical function, i.e. the arguments of the function can be permuted without changing its value.*

A13. ϕ *is a non-decreasing function of each of its arguments.* (A chain cannot be weakened by strengthening any of its links without changing the strength of any of the others.)

T2. ϕ *vanishes if the chain is cut, i.e. if any of the links is of zero strength.* We may alternatively say in this case that there was no causal chain.

A14. *If two consecutive links are replaced by a single link of equivalent strength, then the strength of the chain is unchanged. Formally,*

$$\phi(s_0, s_1, \ldots, s_{n-1}) = \phi(s_0, s_1, \ldots, s_{i-1}, \phi(s_i, s_{i+1}), s_{i+2}, \ldots, s_{n-1}).$$

A15. *A chain is not weakened by* 'omitting' *one of its links, i.e.*

$$\phi(s_0, s_1, \ldots, s_{i-1}, s_{i+1}, \ldots, s_{n-1}) \geqslant \phi(s_0, s_1, \ldots, s_{n-1}).$$

T3. *A chain is no stronger than its weakest link.* (From A10 and A15.)

Definition. Let the maximum possible causal strength be σ. This is either a positive finite number or $+\infty$.

A16. $S(E : F \mid \mathfrak{n}) \leqslant \sigma$, *for any net*, $\mathfrak{n}$. (This axiom is a mere restatement of the definition.)

T4. $Q(p, q, r) \leqslant \sigma$. (From A10 and A16.)

A17. *If any of the links of a chain is of strength* σ, *then it can be 'omitted' in the sense of A15, without strengthening the chain.*

T5. $$\begin{aligned} &\phi(s_0, s_1, \ldots, s_{i-1}\ \sigma, s_{i+1}, \ldots, s_{n-1}) \\ &= \phi(s_0, s_1, \ldots, s_{i-1}, s_{i+1}, \ldots, s_{n-1}). \end{aligned}$$

(From A15 and A17.)

T6. *If every link of a chain is 'cast-iron', then the chain is cast-iron, i.e.* $\phi(\sigma, \sigma, \ldots, \sigma) = \sigma$. (From T1 and T5.)

A18. *A chain of n links all of the same fixed strength,* τ, *where* $\tau < \sigma$, *is as weak as you like if n is large enough. Formally* $\phi(\tau, \tau, \ldots, \tau) \to 0$ *as the number of arguments tends to infinity.*

A19. ϕ *is a continuous function of all its arguments when they are all less than* σ; *and, if* $s_i \to \sigma$, *then the value of the function tends to the value it would have with* $s_i = \sigma$. The reason for the clumsy expression of this axiom is that σ may be $+\infty$.

T7. *If a chain has n links, all of the same strength,* τ, *where* $\tau < \sigma$, *then the chain is as strong as you like if n is fixed and* τ *is close enough to* σ. *Formally, if n is fixed, then*

$$\phi(\tau, \tau, \ldots, \tau) \to \sigma \textit{ when } \tau \to \sigma.$$

(From T6 and A19.)

T8. *There is a function, g, such that, identically,*

$$\phi(s_0, s_1, \ldots, s_{n-1}) = g^{-1}(g(s_0) + g(s_1) + \ldots + g(s_{n-1})).$$

The function, g, is defined for all non-negative arguments not exceeding σ, *and is itself non-negative, continuous and strictly decreasing, and* $g(0) = \infty$, $g(\sigma) = 0$. *We may define g as* $+\infty$ *when its argument is negative.*

Proof. Consider the function $\phi(s, t)$ of just two variables. By A19, A13, A12, and A14, this function may be said to be continuous, monotonic, commutative, and associative. It follows that it is of the form $g^{-1}(g(s) + g(t))$, where g is a strictly monotonic continuous function. The use of the symbol g is justified since A7 and A8 can be satisfied with this function.

The mathematical theorem just invoked was apparently first published by Abel.[1] It was rediscovered several times, such as by Aczél.[2] What it amounts to is that ϕ can be calculated on a suitably calibrated slide-rule.

The results $g(0) = 0$, $g(\sigma) = \infty$, follow from A18 and A19.

Q. E. D.

We may satisfy A6, A7, and A8, which are the only axioms that involve R, by identifying $g(S)$ with R. This identification is no restriction on the explication of Q. As a consequence of this identification we have the following theorem.

T9. *The resistance of a chain is equal to the sum of the resistances of its links.*

A20. *Consider the causal net shown in the diagram below, in which* $P(F) = x, P(G_j \mid F) = p_j, P(G_j \mid \overline{F}) = q_j, P(G_j) = r_j = xp_j + (1 - x)q_j$; $P(E \mid G_1 \vee G_2 \vee G_3) = 1, P(E \mid \overline{G_1} . \overline{G_2} . \overline{G_3}) = 0, P(E) = r, j = 1, 2, 3,$ *and where* G_1, G_2, G_3 *are independent given* F *and also given* $\overline{F}$. *Then the strength of the net is a function of the strengths of the three separate chains, and this function is continuous, monotonic increasing in each variable, commutative* (cf. A12), *and associative* (cf. A14).

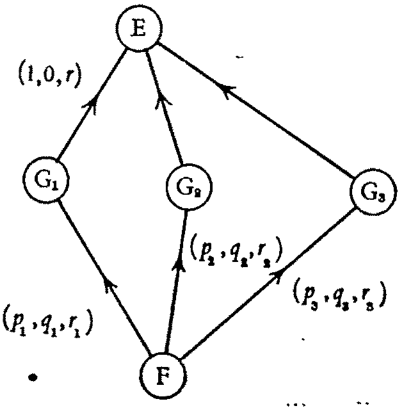

FIG. 1

T10. *The strength of the net of* A20, *generalised to m chains in parallel, is of the form*

$$h^{-1}(h(s_1) + h(s_2) + \ . \ . \ . + h(s_m)),$$

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[1] Neils Henrick Abel, *Oeuvres Complètes*, tome 1, 1881. The paper was Abel's first publication.

[2] J. Aczél, *Bull. Soc. Math. France*, 1948, **76**, 59–64. See also Aczél, *Acta Phys. Acad. Sci. Hungar.*, 1955, 4, 351-362; L. Jánossy, *loc. cit.* pp. 333-349. Or see *Math. Rev.*, **10**, 685, and **16**, 1127–1128.

where $s_1, s_2, \ldots, s_m$ *are the strengths of the individual chains. The function* h *is defined for all non-negative arguments not exceeding* σ, *and is itself non-negative, continuous and strictly increasing, and* $h(0) = 0$, $h(\sigma) = \infty$. (The theorem of the generalised slide-rule implies this theorem, just as it implied T8 above.)

We are now at liberty to call $h(S)$ the strength of a causal net, in place of S, provided we are content to determine the explicatum of S and Q only up to a continuous increasing transformation. It might be thought for a moment that this change of notation would invalidate T9. But since T8 is now true with $g(x)$ replaced by $g(h^{-1}(x))$, we can simply rename this function '$g(x)$' in order to validate T9. With these conventions we have:

T11. *The strength of the net of A20, generalised to m chains in parallel is the sum of the strengths of the individual chains.* When applying this theorem the independence condition mentioned in A20 should not be overlooked.

It appears that the analogy with electric networks is not bad, although the function $f(x)$ turns out later not to be $1/x$, but another self-inverse function.

A21. *In the net of* A20, *with* G_3 *omitted, i.e. with only two chains in parallel, we may regard* $G = G_1 . G_2$ *as a single event, without changing the causal strength of the net.* Note that it would be unreasonable to assume this coalescence property for dependent events, for if we did so we could collapse any net into a single event.

It may be objected that $G_1 . G_2$ is not necessarily a small event. But the strength of a causal net should depend only on its topology, with time-order preserved, and on the various probabilities and conditional probabilities. Hence $G_1 . G_2$ could be a small event in an equivalent network.

Although I think A21 is eminently reasonable, especially in view of later developments, as in Section 9, I believe it to be the weakest part of my argument, and I conjecture that the replacement of this axiom by other assumptions would be the most fruitful method of finding other explicata of tendency to cause, if they exist.

T12. *Identically, if* $p_1 > q_1$, $p_2 > q_2$, $0 < x < 1$, *then*

$$S(p_1 + p_2 - p_1p_2,\ q_1 + q_2 - q_1q_2,\ x(p_1 + p_2 - p_1p_2) + (1 - x)(q_1 + q_2 - q_1q_2))$$
$$= S(p_1, q_1, xp_1 + (1 - x)q_1) + S(p_2, q_2, xp_2 + (1 - x)q_2).$$

This follows at once from T11 by making the identification mentioned in A21.

A22. *$Q(p, q, r)$ is an analytic function when $0 < p < 1$, $0 < q < 1$, $0 < r < 1$, $p \neq q$.*

The only purpose of this axiom is to enable us to extend a formula proved for a large set of values of (p, q, r) to all values except those for which Q may be infinite or indeterminate. I think only an extreme purist would object to A22. It could be avoided by assuming instead that Q is anti-symmetric in the sense

$$Q(p, q, xp + (1 - x)q) = - Q(q, p, xq + (1 - x)p).$$

T13.

$$Q(p, q, r) = u(x) \log \frac{1 - q}{1 - p} = u \left\{\frac{r - q}{p - q}\right\} \log \frac{1 - q}{1 - p},$$

where $u(x)$ is a non-negative analytic function of x.

This theorem, and the next one, will be superseded by T15.

Proof. By A10 and A22, we may replace S by Q in T12, and drop the inequalities $p_1 > q_1$, $p_2 > q_2$. Let

$$\psi(\xi, \eta, x) = Q(1 - e^{\xi}, 1 - e^{\eta}, x(1 - e^{\xi}) + (1 - x)(1 - e^{\eta})),$$
$$p_1 = \exp \xi_1,\ q_1 = \exp \eta_1, \text{ etc.}$$

Then

$$\psi(\xi_1 + \xi_2, \eta_1 + \eta_2, x) = \psi(\xi_1, \eta_1, x) + \psi(\xi_2, \eta_2, x).$$

On putting $\eta_1 = \eta_2 = 0$, and provisionally regarding x as a constant, we get a well known functional equation whose only continuous solution is easily seen to be of the form

$$\psi(\xi, 0, x) = \xi \,.\, u(x),$$

where $u(x)$ is a function of x only. (The only other solutions are in fact non-measurable.[1]) Likewise $\psi(0, \eta, x) = \eta \,.\, w(x)$, where $w(x)$ is a function of x only. Therefore

$$\begin{aligned}\psi(\xi, \eta, x) &= \psi(\xi + 0, 0 + \eta, x)\\ &= \psi(\xi, 0, x) + \psi(0, \eta, x) = \xi u(x) + \eta w(x).\end{aligned}$$

Therefore

$$Q(p, q, xp + (1 - x)q) = u(x) \,.\, \log (1 - p) + w(x) \,.\, \log (1 - q).$$

T13 now follows from A5 combined with the equation

$$r = xp + (1 - x)q.$$

Q. E. D.

A23. *Consider a radioactive particle in a certain state, which I shall call the 'white' state. In any time interval, t, it has probability $e^{-\alpha t}$ of remaining in the white state throughout the interval if it starts the interval in that state.*

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[1] G. Hamel, 'Eine Basis aller Zahlen und die unstetigen Lösungen der Funktionalgleichungen $f(x + y) = f(x) + f(y)$', *Math. Annalen*, 1905, **60**, 459-462. Or see G. H. Hardy, J. E. Littlewood, and G. Pólya, *Inequalities*, Cambridge 1934, p. 96.

If it does not remain in the white state, then it proceeds to another state called here the 'black' state, from which there is no return. Now let F *be the event that the particle is in the white state at the start of an interval of duration* T *and let* E *be the event that it is in the white state at the end of this interval. Then we assume that, if* F *and* E *both occurred,* $\chi(E : F)$ *does not depend on the unit in terms of which time is measured.*

A24. *If* F . E *implies* G, *and* F → G → E *is a chain, then this chain is of the same strength as* F → E.

T14. $R(p, 0, r) = v(r/p) - k \,.\, \log p$,

where $v(x)$ *is a non-negative analytic function of* x, *and* k *is a positive constant.*

Proof. Consider the radioactive particle described in A23. Let $P(F) = x$. The degree to which F caused E is the limit of the strengths of finite chains obtained by breaking up the time interval (0,T) into a 'Riemann dissection' (see A9). Since g is a continuous function (A8) the resistances of these finite chains must also tend to a limit, which we may call the causal resistance from F to E. This must be some function of x, α, and T, say $R^*(x, \alpha, T)$. By A23 we see that for any positive constant, k, the resistance must be equal to $R^*(x, k\alpha, T/k)$. Since this is independent of k it must be of the form of $R^*(x, \alpha T)$.

Now, by a continuity argument, we may generalise T9 to continuous chains, and hence deduce that, for any positive T and U we have

$$R^*(x, \alpha T) + R^*(1, \alpha U) = R^*(x, \alpha T + \alpha U).$$

By giving x the value 1 and subtracting from the equation with arbitrary x, we see that $R^*(x, \alpha T)$ is of the form

$$R^*(x, \alpha T) = v(x) + R^*(\alpha T),$$

where, identically,

$$R^*(\alpha T_1 + \alpha T_2) = R^*(\alpha T_1) + R^*(\alpha T_2),$$

so that $R^*(\alpha T)$ is of the form

$$R^*(\alpha T) = k_1 \alpha T.$$

Now, by repeated use of A24, we see that

$$R(p, 0, xp) = R^*(x, \alpha T),$$

where $p = e^{-\alpha T}$. Thus

$$R(p, 0, r) = v(r/p) - k \,.\, \log p.$$

Q. E. D.

T15. $$Q(p, q, r) = \log(1 - q) - \log(1 - p),$$
$$R(p, 0, r) = -\log p,$$

where the base of the logarithms may be taken as e. $Q(p, q, r)$ *is mathematically independent of* r, *and may be abbreviated to* $Q(p, q)$. *It can be written in other ways:*

$$Q(E : F \mid G) = \log \frac{P(\overline{E} \mid \overline{F} . G)}{P(\overline{E} \mid F . G)} = \log \frac{O(\overline{F} \mid \overline{E} . G)}{O(\overline{F} \mid G)} .$$

$$= W(\overline{F} : \overline{E} \mid G) = - W(F : \overline{E} \mid G),$$

the weight of evidence against F *if* E *does not happen. More precisely,* Q *is uniquely determined only up to a continuous analytic increasing transformation. Among all the explicata there is just one apart from a scale factor (choice of unit), for which theorems T9 and T11 are true.* We lose no real generality, and we gain simplicity, by choosing this explicatum.

Proof. By T13, T14, and A7, we have the identity

$$f(v(r/p) - \log p) = - u(r/p) . \log (1 - p).$$

Let $v(x) = y$, $- \log p = z$, $\log f(y + z) = \rho(y + z)$. Then $\rho(y + z)$ is of the form

$$\rho(y + z) = \rho_1(y) + \rho_2(z).$$

If $v(x)$ is not a constant, we can differentiate and deduce that $\rho(y)$ is a linear function of y, from which we can soon derive that $\log (1 - p)$ is a power of p. Since this is false it follows that $v(x)$ is a constant, and hence also that $u(x)$ is a constant.

The theorem now follows from the remark that the choice of the base of the logarithms is equivalent merely to the choice of units of measurement of strength and resistance. We may call the units 'natural', 'binary', or 'decimal', according as the base is e, 2, or 10. In this paper I shall use natural units. Possible names would be 'natural causats' and 'natural tasuacs'.

Note that the explicatum for Q was by no means obvious in advance, nor was it obvious that all the desiderata could simultaneously be satisfied.

It is interesting to note that, if, contrary to most of the discussion, we assume E to be earlier than F, and if the universe has the 'Markov' property, defined below, then the tendency of F to cause E is zero. This result may very well have been taken as a desideratum, but was in fact noticed only after the explcatum was obtained. By the Markov property is meant here that, for prediction, a complete knowledge of the immediate past makes the remote past irrelevant.

T16. *The relationship between* R *and* S *is symmetrical, namely*

$$R > 0, S > 0,$$
$$e^{-R} + e^{-S} = 1,$$

or equivalently,

$$R = - \log (1 - e^{-S}), \ S = - \log (1 - e^{-R}).$$

Further,

$$R(p, q, r) = \log(1-q) - \log(p-q).$$

This is an immediate corollary of A7 and T15.

Thus the function f is its own inverse, g. It is tempting to permit negative and imaginary values because some of the formalism is faintly reminiscent of Feynmann's formulation of quantum mechanics, but I shall not pursue this matter here.

T17. *If a chain consists of n links whose p's and q's are* (p_i, q_i), *where* $p_i > q_i$, *then its causal strength is*

$$-\log\left\{1 - \prod_i \frac{p_i - q_i}{1 - q_i}\right\}.$$

This follows from T16 and T9.

Before reading the proofs in the present section the reader will probably prefer to read the next two sections, in which some examples are given.

Appendix. Holmes, Moriarty, and Watson (see section 2)

Sherlock Holmes is at the foot of a cliff. At the top of the cliff, directly overhead, are Dr Watson, Professor Moriarty, and a loose boulder. Watson, knowing Moriarty's intentions, realises that the best chance of saving Holmes's life is to push the boulder over the edge of the cliff, doing his best to give it enough horizontal momentum to miss Holmes. If he does not push the boulder, Moriarty will do so in such a way that it will be nearly certain to kill Holmes. Watson then makes the decision (event F) to push the boulder, but his skill fails him and the boulder falls on Holmes and kills him (event E).

This example shows that Q(E: F) and χ(E: F) cannot be identified, since F had a tendency to prevent E and yet caused it. We say that F was a cause of E because there was a chain of events connecting F to E, each of which was strongly caused by the preceding one.

(to be concluded)

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COMMUNICATIONAL EPISTEMOLOGY (I)*

MAGOROH MARUYAMA

IN the history of philosophy the philosopher for long asked himself: 'What is the relationship between THE universe and THE human understanding about it?' This question presupposed the existence of THE universe and of THE human understanding.

THE universe was conceived of in three ways:

(1) As THE material reality outside the human organism, existing independently of the human mind, and perceived through sense organs.
(2) As THE ideal reality, existing independently of the human mind, and of which the human mind can attain understanding, at least in part, by refined thinking.
(3) As a construction of the human mind which is the same for all human beings, though it might be different for other beings.

Those who conceived of the universe in the first way, as THE material reality, can be subdivided into:

(1*a*) Those who interpreted the natural law as independent of the human mind.
(1*b*) Those who interpreted it as a construction of the human mind.
(1*c*) Those who identified the human mind with God, with the natural law, with nature, or, as in mysticism, the human mind with God, natural law and nature.

THE universe of (1*a*), (1*c*) and (2) did not depend on the existence of THE human mind. But the THE-ness of the universe in (1*b*) and (3) depended on the THE-ness of the human understanding. Belief in the possibility and success of interpersonal communication was also dependent on belief in the uniqueness and sameness of the human understanding, except for the subjective qualities of sensations and emotions. This belief was strengthened by the fact that philosophy dealt with conscious and verbally communicable realities[1] and by

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* Received 3.xi.59. The writer is indebted to a referee for numerous stylistic improvements.

[1] M. Maruyama, 'Communicable and Incommunicable Realities', *The British Journal for the Philosophy of Science*, 1959, 10, 50–54

assuming that the encoding of thoughts into language was the same process for all persons and that encoding was the inverse operation of the decoding or interpreting of the other persons' statements. In fact, thoughts were often identified with the language in which they were expressed. Any unsuccessful communication was attributed to mistakes in THE logical process or to discrepancies in the definitions of words. Further, the assumption of the existence of concepts as ideal universals reduced discrepancies in definitions to discrepancies in the coverage by words of the concepts and subconcepts which, in their totality, were assumed to be the same for all persons and all languages.

The sameness of the human understanding, its isomorphism and homogeneity as between persons, was not even questioned or, at least when found, individual differences were regarded as 'subjective' and 'psychological'; they were omitted from philosophical discussions as unworthy of consideration. This was the mechanism by which differences in human understanding were eliminated, enabling epistemology to be founded on the 'universality', 'integrity' and 'absoluteness' of human understanding, thereby making itself universal, integral, and absolute also.

These restrictive assumptions and the classical epistemology based on them are here discarded in favour of a 'communicational epistemology' which recognises human differences. To recapitulate, the restrictive assumptions discarded are:

(1) That human understanding is the same in all persons (or that epistemology is restricted to the study of those parts of human understanding that are the same for all persons).
(2) That logic is the same for all persons (or that 'reasoning pattern' is the same for all persons, if logic is to be reserved for Aristotelian deductive logic).
(3) That concepts are ideal universals identical for all persons.
(4) That epistemology is restricted to the study of conscious and verbally communicable realities.
(5) That the encoding of thoughts into statements, actions, etc. is the same for all persons.
(6) That the decoding of statements, actions, etc., is the same for all persons.
(7) That decoding is the inverse function of encoding.

Hence communicational epistemology is based on:

(1) The existence of various patterns of human understanding.
(2) The existence of various reasoning patterns.
(3) The variability of concepts as between persons.
(4) The existence of unconscious communication as an epistemological subject. The existence of nonverbal communication as an epistemological subject. The existence of incommunicable realities.
(5) The existence of persons with different encoding functions.
(6) The existence of persons with different decoding functions.
(7) The existence of persons whose decoding functions are not the inverse of their encoding functions.

If these seven conditions are not affirmed, then the inauguration of communicational epistemology is not justified. If, on the other hand, any of the conditions are affirmed, inauguration of communicational epistemology is a necessity. But recent developments in cultural anthropology and psychiatry affirm the seven conditions.[1]

Three aspects of classical epistemology have to be modified by acceptance of the seven conditions.

They are:

(1) The relationship between the universe and the human mind.
(2) The relationship between the different understandings of the universe by different persons.
(3) The relationship between the understandings by different persons of each other, or the problem of interpersonal communication.

In this present paper only the third aspect is discussed.

The study of interpersonal communication may be divided into three stages:

(1) Study of the confusions and misunderstandings resulting from contacts between persons with different patterns of thinking, and different encoding and decoding functions.
(2) Study of methods of detecting misunderstandings, modification of encoding and decoding functions, modification of thinking patterns, criteria for correctness of interpretation, successive approximation and its limits of convergence.
(3) Study of the limitations of interpretation and of interpersonal communication.

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[1] M. Maruyama, *Epistemology of Intercultural Understanding: A Study in Behavioral and Communicational Epistemology; Rediscovery of Philosophy as an Open Meta-Science of Interdisciplinary Cross-Induction*, and its *Supplement I*, 1958, 1959. Mimeographed

But the question may be asked: 'Are we not now leaving the domain of philosophy and digressing into the domain of psychology?' The answer is 'No'. The easiest way of proving this is to show that communicational epistemology can be treated, though not necessarily, in the most non-psychological way, i.e. by means of relational algebra, formal operations, etc., within the field of formal logic. Each of the three stages of study given above can be treated formally and rigorously[1] But in this paper, to introduce the subject, only simplified illustrative examples will be given.

1 *A simplified model of misunderstandings due to differences in thinking patterns, encoding and decoding functions*

A typical thinking process in Aristotelian logic is to draw a conclusion from a major and a minor premise, using the law of deduction. A typical operation of an electronic computer (a thinking machine) is to calculate the result of applying to the stored and the newly given information the programmed instructions given in advance. The typical function of a decision element in an electronic computer is to combine two inputs to yield one output determined by the wiring of the element—the internal wiring of the element is called the 'logic' of the element. Mathematically, all these three processes, one by a logician, one by a computer, one by a decision element, can be classified as binary functions of the form $f(x,y) = z$, where f is the law of deduction, programming, internal wiring, etc.

x,y are major and minor premises, stored and input information, two inputs, etc., and z is the conclusion, result of the calculation, the output, etc. The function f is often called a binary operator.[2] Our model of thinking patterns is also a binary function. The process of our model is to produce an internal state (of thinking, feeling, etc.) out of the previous internal state (residual state) and the input, using the operator inherent in the model. The input is either the result of one's own thinking and action, or one's decoding of other persons' statements and behaviour. The residual state and the input are combined and produce a new internal state. The combinations and products can be tabulated in a form similar to that of a multiplication table. This table is the 'thinking pattern', but it may also be called the 'logic', 'algebra', 'internal process function', 'operator', etc. For example, a person A has the residual state (in this case knowledge) 'B has money' and A's interpretation of B's telephone call is 'B is not thinking of inviting A to a concert'; then A's next internal state (thought) is 'B is angry with A', or 'B is impolite', etc., depending on A's thinking pattern (objective, suspicious, etc.), and on the prevailing customs in A's, but not necessarily B's society, etc. Another example: A's residual state is 'My father is a respectable person', and then A suddenly phantasies himself killing his father (an input from his own thinking). The result may be a feeling of guilt and reinforced submission, with the thought 'My father is an impeccable man'; or it may be a realisation that A does not like his father, with the thought 'My father is full of defects', depending on A's personality structure. Thus, the 'thinking pattern' or 'logic' depends on a person's personality as well as on the culture in which he was brought up.

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[1] M. Maruyama, Op. cit.

[2] The word 'binary' is used here in the sense that there are two independent variables or two inputs. This should not be confused with the 'binary' of 'binary digit', or 'bit' in short, used in the electronic computer. The latter refers to a digital system based on the number 2.

We can construct a model of the thinking pattern of an intellectual process, but the 'multiplication table' of intellectual processes becomes rather complex. On the other hand, if we take attitude or desire, for example, as the internal state, it is possible to construct a simpler model to serve as an illustration. This is not to imply that the models are valid only for emotional and psychological states; it is emphasised that the method of constructing the model can be applied also to intellectual processes.

For each combination of the residual state and the input, the new state may not be determined uniquely but may have several alternatives with a probability distribution. For the sake of simplicity, let us assume that the new state is uniquely determined. Different combinations of the residual state and the input may result in a same new state—this possibility is not excluded. An input which is the result of one's own thought or action is called the *return*. An input which is the result of the decoding of another person's statement or action is called the *reception*. For example, a person A codes his contempt for another person B in a form of smiling and B decodes the smile as a sign of A's friendliness towards B. Then 'A is friendly to me' is B's reception. The transformation from A's smile to 'A is friendly to me' is B's decoding. For the sake of simplicity let us assume that the return and the reception have the same effect on the residual state, i.e. as far as the algebra is concerned, it does not matter whether 'A is friendly to me' is a result of B seeing A's smile or is simply a thought which occurred to B without A's presence.

Let us further assume that, in reception, any decoding is transferred to one's own state as input; for example, that my reception of 'You are friendly' makes me friendly. This is called the *assumption of simplicity*. If the state caused by return is again the same state as input, the return is called *regenerative*. For example, the state of desiring to see films making one go to a movie has a regenerative return if this action results in the desire to see more films.

Let us further assume that there is only a finite number of internal states, inputs, and resulting states in any person, and that the states are discrete, i.e. no continuous change of shading from one state to another exists. For example, suppose that the person A has three possible states *c* (contemptuous), *d* (desiring to dance), and *h* (hungry), and that he decodes other person's behaviour in terms of these three states. Let his thinking pattern of 'algebra' be as follows:

	residual state:	*c*	*d*	*h*
input	*c*:	*c*	*c*	*h*
	d:	*c*	*d*	*c*
	h:	*c*	*d*	*h*

This table is called the *state algebra* or *stal* of A. The combination of the input and the residual state is expressed in the form of a product, the left-hand factor representing the input and the right-hand factor the residual state. Thus, $dh = c$ means that the combination of the residual state *hungry* and the input *desiring to dance* results in the new state *contemptuous*. A state algebra has to satisfy the following condition: If S be the set of states, then for any arbitrary elements a and b of S, ab is uniquely determined and is again is S.

It should be noted that neither commutativity nor associativity is assumed, i.e. that neither $ab = ba$ nor $(ab)c = a(bc)$. Any subset of a stal is called a *substal* if it satisfies the conditions of a stal.

So far we have formalised the thinking pattern of only one person. The next step is to examine what happens when two persons with different thinking patterns and different encoding and decoding functions try to communicate with each other. For this purpose let us formulate how A codes his internal state into behaviour. Let c be coded into a smile, d into talking much, and h into putting down a cigarette. Now we have to specify how A decodes other persons' behaviour. It would be natural to assume that since A smiles when A is contemptuous, A interprets anyone else's smile as a sign of contempt: if this were so, A's decoding function would be the inverse of his encoding function. But a person's encoding and decoding functions are not necessarily inverse. An Eskimo living in New York may behave as an Eskimo but may know how to decode an American's behaviour correctly. Or a slave may behave in a slavish way and yet know how to decode his master's behaviour. In these cases, the decoding function is not the inverse of the encoding function. But again for the sake of simplicity, let us assume that the decoding of a person is the inverse of his encoding.

If, for every person within an interacting group, the decoding function is the inverse of the encoding function and all returns have the same effects as the receptions, we call the interaction between the persons *prime*. We will also make the further simplifying assumption that the results of encoding and decoding are uniquely determined, i.e. that the encoding and decoding are *univocal*. When the states are idempotent (i.e. if, when s is a state, $ss = s$) then the return has no effect on the residual state and can therefore be neglected.

We now introduce another person B. Let him have three states: *f* (friendly), *n* (do not believe you) and *d* (desiring to dance). Let his stal be:

	f	*n*	*d*
f:	*f*	*n*	*d*
n:	*f*	*n*	*d*
d:	*d*	*f*	*d*

And let B code *f* into a smile, *n* into talking much, and *d* into putting down a cigarette. We assume that B's decoding is the inverse of his coding. Furthermore, to make the situation more realistic, let us change our first person A into a lady.

We then obtain the following links between the two persons through their behaviour:

	c	*d*	*h*	A	Behaviour	B		*f*	*n*	*d*
c:	*c*	*c*	*h*	*c* (contempt)	smile	*f* (friendly)	*f*:	*f*	*n*	*d*
d:	*c*	*d*	*c*	*d* (desiring to dance)	talks much	*n* (do not believe you)	*n*:	*f*	*n*	*d*
h:	*c*	*d*	*h*	*h* (hungry)	puts down cigarette	*d* (desiring to dance)	*d*:	*d*	*f*	*d*

As all states are idempotents, the returns may be neglected. This diagram with the stals of A and B flanking their behaviour pattern is a very simple model of the interaction between two persons. A model of interpersonal interaction thus schematised is called an "algebra of interpersonal interaction' or intal. When an intal is given and the initial states of the persons are specified, we can compute the sequence of interactions between the persons.

For example, let us assume that A's initial state is *h*, that B's initial state is *n* and that A expresses herself first. The course of interaction develops as follows: A puts down her cigarette, an action which B interprets as indicating that A desires to dance, i.e. his reception is *d*. The combination of reception *d* with B's initial state *n* produces *f*, a state of friendliness which B expresses with a smile. This is received by A as *c*, an expression of contempt. The reception *c* combined with A's residual state *h* produces the state *h*: A puts down her cigarette. This gives reception *d* on B. But this time B's residual state is *f*, and the combination of reception *d* and residual state *f* is the new state *d*; B is in the state of desiring to dance. He expresses this state by putting down his cigarette, which makes reception *h* on A. The result, the combination of reception *h* with residual state *h*, is again *h*. From this point the process repeats, A and B putting down their cigarettes alternately, while A is always hungry and thinks that B is hungry, while B is always anxious to dance and thinks that A is also anxious to dance. This kind of misunderstanding is typical of intercultural contact situations.

But if the initial states of the two persons are different from those in the above example, the sequence of interactions may take a different course and may end with a different final behaviour. If, for example, A starts with *c* and B with *d*, and if A expresses herself first, then, after some computation the final state reached is that A is always contemptuous and smiles believing that B is hungry while B is always anxious to dance and puts down his cigarette, thinking that A is friendly. This also is a typical pattern of intercultural misunderstanding.

Our example ended with situations in which an equilibrium is established, each person remaining unchanged. But there are intals which never produce equilibrium regardless of the choice of the initial states of the two persons and regardless of who expresses himself first. Suppose we change the stals of the preceding intals as follows:

A	*c*	*d*	*h*
c:	*d*	*h*	*d*
d:	*h*	*h*	*c*
h:	*d*	*c*	*c*

B	*f*	*n*	*d*
f:	*d*	*d*	*n*
n:	*d*	*f*	*f*
d:	*n*	*f*	*n*

and assume that there is no return. Then we find that both A and B ultimately reach a cycle of three states. The pattern of interactions and misunderstandings as determined by the structure of the intal (which is composed of the stals and the encoding and decoding functions of the persons involved) can be elaborated. The procedure, as we have seen, is a strictly logical and formal process. Though prosaic interpretations have been used in the illustrative examples, the interactions can be formalised. It is also possible to construct electronic robots with given stals, encoding, and decoding functions, and to make them play out interpersonal misunderstandings. The wiring diagram of such robots is available.[1]

We have seen how the first stage of our inquiry into communicational epistemology—misunderstandings due to differences in thinking patterns, encoding and decoding functions—can be formalised. We next present a formal treatment of the second and third stages of our inquiry—correction of misunderstandings and the limitations of interpersonal understandings. Empirical examples of misunderstandings in interpersonal and intercultural communications will then be given.

(*to be continued*)

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[1] M. Maruyama, 'The Circuitry of Electronic Robots to play out Interpersonal Misunderstandings.' 1957. Mimeographed

DISCUSSIONS

A SECOND NOTE ON THE PRINCIPLE OF MINIMUM ASSUMPTION

ALTHOUGH I do not believe that Professor Kapp has satisfactorily met the points I have raised against his thesis[1] I shall not go over the same arguments again. It is better left to the judgment of the interested reader to decide whether the proposed principle of minimum assumption has held the ground against these points.

There was, however, one point to which Professor Kapp has made no reference at all in his reply and which by itself would suffice to show the inadequacy of his principle. I am referring to the last objection raised in my note that Kapp's principle lacks constraining conditions and therefore, like all minimal (or maximal) principles which lack constraining conditions, is indeterminate. I now realise that this was a rather curt statement and its significance none too clear. I should like, therefore, to elaborate slightly upon it.

To justify this further imposition on the patience of the readers of this journal I venture to suggest that the point I am about to make may be of more general interest. It provides a reason why all assertions to the effect that nature is governed by maximum simplicity are indefensible.

Let us consider Fermat's principle of least time, from which it is said one can determine the path of a light ray traversing different media and suffering successive refraction. Let us state it as follows: 'The path travelled by light going from one point to another, traversing different media, is that which requires least time.' Stated just like this, the path of light would be indeterminate. The assumption which would conform best with the principle would be that the velocity of light was always infinite. Hence, whatever the shape of the path followed by light, it will travel from any point A to point B instantaneously. But of course Fermat's principle has to be applied in conjunction with the constraining conditions, which are: the velocity of light is finite and is uniquely determined by the nature of the medium traversed. The time spent in transit is now wholly dependent on the shape of the path. Given the velocity of light in the various media we can construct a unique course which is to be followed so as to minimise the duration of the journey.

Let us now consider again Kapp's principle: 'In Physics the minimum assumption always constitutes the true generalisation.' Clearly, the state of affairs which would conform best to this principle would be that in which there were no forces, no action, neither matter nor motion—a universe in which nothing existed but still void. Professor Kapp's Cosmic Statute Book would then contain the minimum number of entries; that is, no entries at all.

Undoubtedly then, Kapp's principle as it stands is incomplete. Lacking appropriate constraining conditions, it is not a principle of minimum assumption but a principle of *no* assumption, which is continually violated by the presence of any phenomena.

The only doubt one may still have is: could there be a remedy for this situation ? And the answer would have to be: by stipulating that nature has certain fixed objectives and that Ockam's razor is only to be applied to the way in which these objectives are achieved but not to the objectives themselves. We would have to distinguish between 'ends' and 'means' in nature. The 'ends' of nature are set but the 'means' must comply with the principle of minimum assumption.

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[1] This *Journal*, 1960, 10, 55-62

This basic distinction has to be made whenever it is claimed (irrespective of the particular form in which it is claimed) that in nature maximum simplicity obtains. It is always understood that maximum simplicity applies not to all phenomena but to those which are instrumental in bringing about the objectives of nature.

Thus, for example, Galileo's dictum, 'Nature does not do by many that which can be done by a few'. It implies that there are certain tasks to be 'done' by nature and these tasks are executed with maximum efficiency and economy. One could, however, not speak of efficiency and economy if there were no specific tasks to accomplish.

The difficulty then is obvious. One cannot seriously defend a classification of phenomena into 'ends' and 'means'. Hence, already on this account the principle of minimum assumption is untenable.

Finally, it should be clear that this difficulty does not defeat all the attempts to formulate a principle of simplicity. It obviously does not affect the view in which the principle is not a feature to which the universe conforms but a rule for the scientist to conform with. 'Given a certain phenomenon, choose the hypothesis of maximum simplicity.' The constraint here is set by the observer. The 'ends' are the explananda and the 'means', which have to be kept at a minimum complexity, are the explanations.

But even a claim that nature itself is simple can escape the difficulty provided no maximum simplicity is stipulated. There would be no *a priori* objection on the above grounds to a thesis that all the relationships in nature are simple enough to be representable by a mathematical expression not more complex than F—where F is a given functional relationship. But no version of the claim that this universe of ours is the simplest possible can be defended.

G. Schlesinger

Reply to Note by G. Schlesinger

For the convenience of readers who do not have easy access to the correspondence between Mr Schlesinger and myself in the *Journal* of May 1960, I shall begin by repeating some of the implications of the Principle of Minimum Assumption in all their uncompromising starkness. If this principle is true, all general statements in physics are either tautologies or reducible; they can be inferred from wider generalisations. In this sense they can all be explained. The laws of physics are of a different kind from those laws that demand certain things and prohibit others. There is nothing in physics to which the metaphor of a cosmic statute book applies. Nature cannot be regarded as a legislator. There is no law or principle in physics that makes for specified order in the sense in which the laws of a country make for specified order.

The question with which this correspondence is concerned is whether the Principle of Minimum Assumption is valid. To formulate it with philosophical precision would require a long and cumbersome dissertation, but this is not necessary. The question can be paraphrased: Is there, or is there not, such a thing (metaphorically speaking) as a cosmic statute book ? In my youth I took it for granted, and without giving any thought to the matter, that there must be such a book and that it must contain a great many entries. In middle age I was seriously questioning this. Today I am almost satisfied that there is no such book at all. Mr Schlesinger thinks there probably is, but I am sure he would agree that it cannot contain as many entries as it seemed to in the past.

The disagreement therefore centres around the 'some or none' issue; entries or no entries. But it is important also to note well the point on which we are agreed. This is that the question deserves serious study and calls for disciplined thinking and discussion; I myself would regard it as one of the two or three most basic unanswered questions in the philosophy of science. The trouble is that both possibilities—'some' and 'none'—have unwelcome implications, which explains the unpopularity of the question. It is easier to change the subject or to suggest vaguely that the correct answer may lie somewhere between 'some' and 'none' than to face the difficulties raised by each possibility. I welcome it that Mr Schlesinger is prepared to face the difficulties.

Let me now give two reasons why my own satisfaction with the Principle of Minimum Assumption, although ninety per cent, is not a hundred per cent.

Firstly, there is Mr Schlesinger's own objection that the principle implies a complete absence of restraining conditions. This is true by definition. Mr Schlesinger says that, in the absence of restraining conditions, the material universe would be indeterminate. I agree. But I could also imagine this argument being used against Mr Schlesinger; for the material universe *is* indeterminate. The movement of individual electrons conforms to the uncertainty principle; the moment of time when a given atom of radiation disintegrates is not determined by anything in the existing state of affairs. There are certainly events that occur in the absence of restraining conditions. However an attempt to counter Mr Schlesinger by quoting these would only be to score a cheap debating point. While it is true that individual micro-events are indeterminate it also follows from the laws of probability that the effect of a large collection of events is predictable. If the cosmic statute book contains nothing else it should, according to Mr Schlesinger's argument, contain at least those fractions that define the probabilities of certain classes of events. I do not think that Mr Schlesinger's argument is easy to refute.

Secondly, there are certain statements in physics that appear to be incapable of reduction, of explanation; no one has succeeded in showing that they are implicit in wider generalisations or that they are tautologies. Among them are the conservation laws, a handful of cosmic constants, some quantitative definitions of probability. These suggest a cosmic statute book. My belief that they will one by one disappear, as so many other generalisations have done in the past, is based only on faith and not on facts. This does not allow me to be a hundred per cent satisfied with my own Principle.

But against all this, let me now say why my doubts about the validity of this Principle are really very slight.

Firstly, the last few centuries have seen a most consistent and drastic elimination of the sort of 'laws' that could originally have led to the notion of a cosmic statute book. The number that seem to remain look, by contrast, so meagre, so trivial, so odd, that I find it more rational to believe that even these will be eliminated in due course; that it will eventually be seen that there never were such things as 'Nature's Laws.'

Secondly, there is a pragmatic consideration. To accept that a generalisation in physics implies a cosmic statute book is to treat it as inexplicable; it is to abandon all hope that it may some day be understood and correlated with other generalisations. This consideration suggests that all such general statements in physics should be critically examined with a view to their possible elimination from the cosmic statute book. Whether the Principle of Minimum Assumption is valid or not, physicists do in fact often act as though they believed in it, and their action has often proved fruitful.

Years ago it occurred to me to examine critically two generalisations all too readily treated as though they were entries in our imaginary book. They were: 'The contents of the material universe had to originate at a specific point in time, a finite number of years ago', and 'Every elementary component of the material universe shall continue to exist for an infinite time.' It was my discovery of how much that was puzzling could be explained by denying these generalisations that increased my fifty per cent satisfaction with the Principle of Minimum Assumption to its present ninety per cent.

REGINALD O. KAPP

METHODOLOGICAL AND EPISTEMOLOGICAL INDIVIDUALISM

HAYEK and Popper have something important to say on methodological individualism. I have been reading more recent writings on this subject, and I wish to report that the game's not worth the candle. The more recent writings I have read comprise eight contributions to this *Journal*, various contributions to *Philosophy of Science*, *The British Journal of Sociology*, the *Aristotelian Society Supplementary Volumes*, and *The Journal of Philosophy*, and parts of several books.[1]

Two different principles have the attractive designation 'methodological individualism'. Hayek applies the term to a methodological principle, and Popper applies it to an epistemological principle. Hayek's principle is one about the *methods* that should be employed in gathering information and forming theories: our *data* in the social sciences are 'the relations between individual minds which we directly know'.[2] Popper's principle is a blanket epistemological principle: whatever methods we have used, 'we should never be satisfied by an explanation in terms of so-called "collectives" (states, nations, races, etc.)'.[3] Hayek's principle is about how we should start our enquiries, Popper's about how we should finish them: Hayek says that the methodological individualist 'systematically *starts from* the concepts which guide individuals in their actions ';[1] Popper says that methodological individualism 'rightly insists that the "behaviour" and the "actions" of collectives, such as states or social groups, must be *reduced* to the behaviour and to the actions of human individuals '.[2] Hayek's principle is synthetic, Popper's analytical: Hayek says that the social sciences 'do not deal with "given" wholes but their task is to *constitute* these wholes by *constructing* models from the familiar elements ';[3] Popper says that 'institutions (and traditions) must be *analysed* in individualistic terms '.[4] It is not surprising that there should be this series of related differences between Hayek and Popper, for Hayek is interested only in how theories are formed and says nothing about testing theories, and Popper says:

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[1] Many of the references are given in footnotes to J. W. N. Watkins, 'Historical Explanation in the Social Sciences', this *Journal*, 1957, **8**, 104

[2] F. A. Hayek, *The Counter-Revolution of Science*, Illinois, 1952, p. 57

[3] K. R. Popper, *The Open Society and its Enemies*, London, 2nd edn., 1952, Vol. 2, 98

> The question, 'How did you first *find* your theory?' relates, as it were, to an entirely private matter, as opposed to the question, 'How did you *test* your theory?' which alone is scientifically relevant.[5]

I have read eight articles and notes by Watkins on methodological individualism, five of them in this *Journal*. He does not distinguish between methodological individualism properly so called and what I have called epistemological individualism. He writes as though Hayek and Popper mean the same thing by 'methodological individualism'. He advocates a 'methodological individualism' which is a jumble of Hayek's principle and Popper's principle. The jumbling process has distorted both of these principles without producing any new principle.

Hayek says that 'in the social sciences our data or "facts" are themselves ideas or concepts '[6] in the minds of individuals. Even statistics, which one might expect an economist to regard as furnishing important data, are (unless they are 'concerned with the attributes of individuals ')[7] quite irrelevant to social theory, since they do no more than 'provide us with the data to which our theoretical generalizations must be applied to be of any practical use '.[8] This may be sound or unsound, but it is at least unqualified methodological individualism.

Watkins distorts Hayek's methodological individualism when he says:

> The assertion that knowledge of social phenomena can only be derived from knowledge about individuals requires one qualification. For there are certain overt features which can be established without knowledge of pyschological facts, such as the level of prices, or the death-rate (but *not* the suicide-rate).[9]

He gives a circular definition of' overt feature ' as ' something which can be ascertained without referring to people's dispositions, etc.'[10] In other words, Hayek's universal principle applies to all social phenomena except the ones it does not apply to.

Hereafter in deference to Popper I shall drop the term 'epistemological individualism'. Popper says that methodological individualism is the doctrine that 'we must try to understand all collective phenomena as due to the actions, interactions, aims, hopes, and thoughts of individual men, and as due to traditions created and preserved by individual men '.[1] At first sight it is puzzling that Popper should superadd this requirement to what he so often says is his sole requirement, testability. Thus it is not easy to reconcile his methodological individualism with his statement that ' the question, " How did you *test* your theory? " alone is scientifically relevant '. I believe that to solve this puzzle we have to examine Popper's views on the purpose of social science. This purpose is twofold: first, to form ' sociological laws or hypotheses which are analagous to the laws or hypotheses of the natural sciences ';[2] second to give ' the explanation of some regularity or law '.[3] The solution to the puzzle is that in a law all that matters is testability, but an explanation must comply with the principle of methodological individualism. Laws do not need to comply with this principle. Most of Popper's examples of sociological laws[4] contravene the principle of methodological individualism, for instance ' You cannot have a centrally planned society with a price system that fulfils the main functions of competitive prices ' and ' You cannot have full employment without inflation '.

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[1] Op. cit. p. 38 (my italics) [2] Op. cit. p. 91 (my italics)

[3] Op. cit. p. 56 (my italics of second word italicised)

[4] Op. cit. p. 324 (my italics)

[5] K. R. Popper, *The Poverty of Historicism*, London, 1957, p. 135

[6] Op. cit. p. 36 [7] Op. cit. p. 61 [8] Op. cit. p. 63

[9] J. W. N. Watkins, 'Ideal Types and Historical Explanation', this *Journal* 1952, 3, 28 (Watkins's italics)

[10] Ibid.

Watkins distorts Popper's principle (and shows that he forgets the purpose of science) when he down-grades Popper's ' laws ' to ' unfinished or half-way [*sic*] explanations ':

> According to this principle, the ultimate constituents of the social world are individual people who act more or less appropriately in the light of their dispositions and understanding of their situation. Every complex social situation, institution, or event is the result of a particular configuration of individuals, their dispositions, situations, beliefs, and physical resources and environment. There may be unfinished or half-way explanations of large-scale social phenomena (say, inflation) in terms of other large-scale phenomena (say, full employment); but we shall not have arrived at rock-bottom explanations of such large-scale phenomena until we have deduced an account of them from statements about the dispositions, beliefs, resources, and inter-relations of individuals. (The individuals may remain anonymous and only typical dispositions, etc., may be attributed to them.) [5]

Watkins gives two illustrations of theories that his principle would prohibit. To be satisfactory, these would have to be illustrations of either (*a*) explanations that do not comply with the principle or (*b*) ' half-way ' explanations that cannot be reduced to explanations that comply with the principle. They both prove, on examination, to be illustrations of (*c*) ' half-way ' explanations that can be so reduced. As ' half-way ' explanations they are laws and hence acceptable in Popper's view; and, even if Popper required laws to be reducible to individualistic statements (which he does not do), these explanations would still be acceptable to him since they are so reducible. The first illustration is:

> An example of such a superhuman, sociological factor is the alleged long-term cyclical wave in economic life which is supposed to be self-propelling, uncontrollable, and inexplicable in terms of human activity, but in terms of the fluctuations of which such large-scale phenomena as wars, revolutions, and mass emigration, and such psychological factors as scientific and technological inventiveness can, it is claimed, be explained and predicted.[6]

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[1] Popper, *The Poverty of Historicism*, pp. 157-158

[2] Ibid. p. 62

[3] Ibid. p. 122

[4] Ibid. pp. 62-63

[5] This *Journal*, 1957, 8, 105-106

[6] Ibid. pp. 106-107

This illustration is unsatisfactory unless Watkins can produce an economist who purports to propound a theory that is inexplicable in terms of human activity. This he does not do. It is worth noting that in a footnote to a very similar passage in his 1952 article Watkins says that he wrote the passage with the Russian economist Kondratieff in mind. As he does not repeat the footnote in his later article, he possibly no longer believes that Kondratieff illustrates his point. In the article referred to by Watkins, Kondratieff does not say that the long-term cyclical wave is 'self-propelling, uncontrollable, and inexplicable in terms of human activity'. What Kondratieff does say is:

> In asserting the existence of long waves and in denying that they arise out of random causes, we are also of the opinion that the long waves arise out of causes which are inherent in the essence of the capitalistic economy. This naturally leads to the question as to the nature of these causes. We are fully aware of the difficulty and great importance of this question; but in the preceding sketch we had no intention of laying the foundations for an appropriate theory of long waves.[1]

The second illustration is: 'Marx, for instance, professed to believe that feudal ideas and *bourgeois* ideas are more or less literally generated by the water-mill and the steam-engine'.[2] But this is a travesty. What Marx says is fully in accord with methodological individualism:

> M. Proudhon the economist understands very well that men make cloth, linen or silk materials in definite relations of production. But what he has not understood is that these definite social relations are just as much produced by men as linen, flax, etc. Social relations are closely bound up with productive forces. In acquiring new productive forces men change their mode of production; and in changing their mode of production, in changing the way of earning their living, they change all their social relations. The hand-mill gives you society with the feudal lord; the steam-mill, society with the industrial capitalist.[3]

There are further confusions in what Watkins has to say. Popper clearly distinguishes methodological individualism from methodological psychologism:

> The mistake of psychologism is its presumption that this methodological individualism in the field of social science implies the programme of reducing all social phenomena and all social regularities to psychological phenomena and psychological laws.[4]

Watkins advocates a psychologistic methodological individualism in his 1952 article in this *Journal*, and an anti-psychologistic methodological individualism in his 1957 article in this *Journal*. In 1952 he gave a classic statement of methodological psychologism:

> From this truism I infer the methodological principle which underlies this paper, namely, that the social scientist can continue searching for explanations of a social phenomenon until he has reduced it to psychological terms.[5]

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[1] Kondratieff, in source cited by Watkins, pp. 41-42

[2] This *Journal*, 1957, 8, 111

[3] Marx, *The Poverty of Philosophy*, London, Martin Lawrence, not dated, p. 92

[4] Popper, *The Open Society*, Vol. 2, p. 98

[5] This *Journal*, 1952, 3, 28-29

And in 1957, without any apparent awareness that he had changed his views, he said:

> Another [misunderstanding] of methodological individualism is that it has been confused with a narrow species of itself (Popper calls it ' psychologism ')....[1]

There is of course no reason why a writer's views should not change over the years, but when they change he should not confuse his readers by leading them to suppose that his new views are re-statements of his old views.

In the same 1957 article there appears an economic digression which Watkins might have supposed runs counter to any version of methodological individualism:

> ... it is very doubtful whether an economist can ever *show* that an economic system containing negative feed-back will be stable. For negative feed-back may produce either a tendency towards equilibrium, or increasing oscillations, according to the numerical values of the parameters of the system. But numerical values are just what economic measurements, which are usually ordinal rather than cardinal, seldom yield. The belief that a system which contains negative feed-back, but whose variables cannot be described quantitatively, is stable may be based on faith or experience, but it cannot be shown mathematically.[2]

Watkins does not appear to have replied to what I consider the most cogent of the criticisms of methodological individualism, Maurice Mandelbaum's ' Societal Laws ', which appeared in this *Journal* in 1957.[3] This is odd, for Watkins has replied to most of the criticisms levelled against his versions of methodological individualism, and has replied to a criticism which appeared in this *Journal* six months later than Mandelbaum's article. Mandelbaum gives a clear-headed classification of sociological laws into four classes and shows point by point that his fourth class (which he calls ' abstractive-functional societal laws '), though prohibited by the principle of methodological individualism as it is stated by Watkins, are free from the defects that Watkins attributes to the theories his principle prohibits.

It is true that Mandelbaum is non-committal on whether such laws exist:

> Whether such laws have been found, or whether we have reason to believe that they may be found, is not the question which I have proposed for this discussion.[4]

But Mandelbaum also says:

> Among the examples of attempts to formulate such laws we may cite the following: statements concerning relationships between modes of production and marriage systems; between size of population and political organisation; between forms of economic organisation and political organisation; or, to cite a classic study of Tylor's (which has been amplified and elaborated by Murdock in his *Social Structure*) between certain specific aspects of marriage systems, e.g. rules of residence and, rules of descent.[5]

Mandelbaum's distinction between ' laws ' and ' attempts to formulate such laws ' is unsatisfactory in terms of Popper's refusal to accept a distinction between hypotheses and laws.[6] If attempts have been made to formulate laws, then hypotheses have been advanced. But even if Mandelbaum had not in effect stated that hypotheses of his fourth class have in fact been made, Watkins would still need to reply, for the point at issue between Mandelbaum and himself is about a methodological prescription. Watkins says that certain kinds of theory may not legitimately be postulated as they have certain defects, and Mandelbaum says that one class of these theories are free from the defects Watkins attributes to them.

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[1] This *Journal*, 1957, 8, 111-112

[2] Ibid. p. 114

[3] ' Societal Laws ', this *Journal*, 1957, 8, 211

[4] Ibid. p. 222

[5] Ibid. p. 221

[6] For instance, Popper, op. cit., Vol. 2, pp. 260-261

K. J. Scott

Victoria University of Wellington

Note on the Teaching of History and Philosophy of Science

Department of History and
Philosophy of Science,
University of Aberdeen

Sir,

I should be obliged if you would correct a statement in the November issue of the *Journal* to the effect that at the University of Aberdeen the balance in the teaching of History and Philosophy of Science is ' Largely in the History of Science '. Though the policy of the Department, in consultation with the Faculties, is to maintain the balance as evenly as possible, it is the case that the philosophical aspects somewhat predominate. Even in the form set out in the article the excess of history is very slight; but what may not have been made clear in my report to Dr Mays is that whereas the weekly philosophy course covers the whole session the history course is only for two terms. Also the evening staff Seminars generally have a philosophical balance.

In conclusion I should like to emphasise that in *all* the courses opportunity is taken to point out the danger of studying either branch of the subject in complete isolation from the other.

Yours faithfully,

William P. D. Wightman

Reply to Note by W. P. D. Wightman

Department of Philosophy,
The University,
Manchester, 13

Sir,

In reply to Dr Wightman's letter I ought to say that I used the category ' History of Science ' in a fairly wide sense to cover History of Science pure and simple as well as philosophical discussions of science as seen in their historical perspective. I think most of Dr Wightman's discussions are of the latter type. Perhaps the real difficulty arises from my attempt to divorce these two aspects of the subject. It is difficult to do this in practice.

Yours faithfully,

W. Mays

REVIEWS

WORDS AND THINGS[1]

Now that some of the dust stirred up by its first appearance has settled it should be possible to take a somewhat cooler look at Mr Gellner's *Critical Account of Linguistic Philosophy* than was accessible to its first strenuous supporters and opponents. Both its style and its content show it to be a pamphlet rather than a fully-fledged philosophical treatise. It is written in a passionate, hectic and none too orderly way; it is protracted beyond its natural length by a good deal of repetitiousness and its sociological excursions, though entertaining enough as a kind of dinner-table conversation, are only marginally relevant to the book's philosophical intentions. As for its content: Mr Gellner does not exert himself to identify very clearly the precise object of his attack, linguistic philosophy remains to the end a pretty shapeless affair, and his method of condensing basic assumptions out of the philosophical atmosphere does not have much to do with the rules of evidence that ordinarily prevail in this matter. He relies on intuition and hearsay, on revelatory word of mouth, parentheses and stray journalistic lettings-down of hair, rather than on the more solid published endeavours of the objects of his criticism, for a basis on which to erect his account of their fundamental ideas about philosophy. On the rare occasions when he does settle down to the task of examining some concrete piece of philosophical work by a linguistic philosopher the result is sometimes frivolous (cf. p. 217 '" uses but not sentences refer " (I have a terrible feeling I may have got the phrasing of this latter idea wrong)'.) And it is sometimes incompetent (for example, his treatments of the paradigm case argument, of Wittgenstein's rejection of private languages and of Strawson's critique of the theory of descriptions). In a way he has fallen between two stools; a serious critical investigation of a body of philosophical ideas on the one hand, such as Mill's *Examination of Hamilton's Philosophy*, and a satirical, Lytton Strachey-style demolition of an intellectual fashion, such as Fisher's *Our New Religion*, on the other. A shorter and more enjoyable book would have come from concentration on the second task. To have embarked on the first he would need to have done a good deal more work than he has.

The linguistic philosophy he is concerned with is an amalgam of two rather different components: the later philosophy of Wittgenstein and the Austinian philosophy of ordinary language that has dominated post-war Oxford. The adherents of these two bodies of opinion often have a marked disregard for one another. But there are some common assumptions that are perhaps most easily defined in terms of the considerable dependence of both groups on the philosophy of G. E. Moore. The very general convictions which Mr Gellner is principally interested in can, I think, most clearly be sorted out under the following three heads (though he does not himself expound linguistic philosophy in quite this way): (*a*) a doctrine of commonsense naturalism about the world and about language, (*b*) an insistence on the authority of ordinary language with which is bound up the view that bad, old, metaphysical philosophy consists in subtle and seductive distortions of the normal meanings of words and the view that the proper task of a reformed and disillusioned philosophy is the detection and explanation of these errors, and (*c*) a kind of flight from theory, an abdication of intellectual responsibility which refuses to propound general philosophical theses on account of the bewilderingly ineffable polymorphism of language.

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[1] By Ernest Gellner. Gollancz. 1959. 25s.

The merit of this classification is that it conveniently brings out Mr Gellner's main grounds for dissatisfaction with linguistic philosophy. Commonsense naturalism, he claims, is simply a dogma, assumed or insinuated in terms that suggest that no sensible person could doubt it. It fails to deal with the traditional obstacle to naturalism, the epistemology which takes subjective experience to be the only secure basis for knowledge, by simply ignoring it. And in contrast to previous varieties of naturalism it is quite indifferent to the achievements of science, natural or social. This kind of complacent quietism is, however, more conspicuously the offence of school's devotion to ordinary language. All past philosophy is dismissed as confusion, which it is the only possible office of a reformed philosophy to dispel. The outcome is general passive indifference to intellectual creativeness and enterprise and an insincere pretension to neutrality. Succumbing to the authority of ordinary speech the linguistic philosopher neglects his essential duty, that of critically investigating the validity of our inherited stock of ideas and beliefs. In his flight from theory, finally, the linguistic philosopher abandons the search for unity, order, and coherence, which is an indispensable requirement for any serious intellectual undertaking. Such a position underwrites evasiveness and esotericism, and replaces explicit argument by furtive insinuation. In short, dogmatic commonsense and a timid and scholastic conceptual conservatism disdain any thinking that is creative or genuinely critical. They induce the facile acceptance of established ideas and beliefs. The repudiation of theory leads to evasiveness and systematic intellectual dishonesty, to an anti-intellectual obsession with trivial detail and, in the end, to simple boredom.

The first thing to be noticed is that these three doctrines are not very closely logically bound up with one another. A respect for ordinary language naturally presupposes an acceptance of commonsense naturalism but the converse relation does not hold and both of these views are quite independent of the contention that there can be no general philosophical propositions. A single example is enough to make the point, that of Professor Ayer who has no hostility to conceptual innovation and is certainly no opponent of general theories in philosophy and yet who says 'The philosopher has no right to despise the beliefs of commonsense. If he does so, he merely displays his ignorance of the true purpose of his enquiries.' A difficulty here is that the acceptance of commonsense is an exceedingly vague commitment. Mr Gellner's versions of it—'The world is what it is', 'The world is what it seems'—do nothing to remove the vagueness. Is it a disposition to reject any philosophical doctrine that *conflicts* with commonsense or, more stringently, that merely *adds to* it, by, for example, countenancing the existence of sense-data? And just how far does the authority of commonsense extend? If it holds that men can often rightly be blamed for what they do is it also committed to the occurrence of uncaused acts of will; if it maintains that we really perceive independent material things in space does it also contend, with Professor Price's splendid Naive Realist, that visual sense-data are literally identical with the surfaces of material things? While commonsense naturalism about the world in general is vague, and conspicuously left in that condition by Mr Gellner, commonsense naturalism about language, the view that it is a rule-governed behavioural activity in the common world, seems just obvious. No one could seriously deny that it is at least this. It is to go further, though, to say with Wittgenstein, that it cannot conceivably be anything but this. But that language is a social art—the phrase is Professor Quine's—is hardly a belief peculiar to linguistic philosophers.

Mr Gellner's main objection to commonsense naturalism seems to be that it is simply a dogma, advanced, not on logical grounds, but in an exclusively rhetorical fashion with the aid of scornful utterance and raised eyebrows. This is surely incorrect. G. E. Moore offered a large number of arguments in its defence, conveniently put together in the second chapter of Mr A. R. White's book on him. Professor Quine, amongst others, has made use of Neurath's famous image about repairing our conceptual boat with its own timbers. Others again have preferred to augment the natural authority of commonsense by attempting to refute the traditional sceptical arguments against it, the Cartesian epistemology that Mr Gellner holds linguistic philosophers to have simply ignored. It is understandable that, in trying to arrive at the philosophical opinions of Professor Austin, he should have relied on information about unpublished statements. But in that case it is odd that he should have neglected Austin's famous lectures on 'Sense and Sensibilia', delivered on a number of occasions in post-war Oxford. As to the doctrine of the necessary publicity of language, this was elaborately argued for by Wittgenstein and has been elaborately examined since the publication of *Philosophical Investigations;* it has been criticised by Mr Strawson and Professor Ayer, defended by Professor Malcolm and Mr Rhees. Mr Gellner's own account of Wittgenstein's reasons for his view is perfunctory in the extreme. In short, as far as commonsense naturalism is concerned, Mr Gellner fails to identify the theory to which he is opposed, is mistaken in claiming that it has been simply taken for granted as a dogma of the school and fails seriously to consider, let alone refute, the arguments on which it rests. Above all his objections are deprived of force by his failure to indicate what alternative he has in mind. (This is, indeed, a prevailing feature of his book.) A kind of Lockean scientism is occasionally hinted at, the view that commonsense is undercut by the discoveries of natural science, as in his attempt to reconstitute Eddington's famous two tables, but it is never explicitly argued for.

Belief in the authority of ordinary language is much more central to linguistic philosophy than commonsense naturalism. Mr Gellner picks out four arguments as pillars of linguistic philosophy and holds the belief in the primacy of ordinary language to rest on three of them: the argument from paradigm cases, the argument about the necessary applicability of each of any pair of contrasted terms and what he calls the generalised form of the naturalistic fallacy which says that actual use is correct use. His counter-arguments are not very impressive. To show that the paradigm-case argument is ' immensely silly ' he refers to the distinction between connotation and denotation and claims that it confuses the two. But this is much too simple. The paradigm-case argument is, of course, a variant of a traditional empiricist line of thought: the theory that complex ideas must be analysable into simple ideas being turned into the view that we can only discover what a word connotes or means by reference to the things denoted by it or by the other words with whose connotation its own is identified. On this interpretation the scope of the argument is limited to ostensive words. It may be difficult to decide just which words are ostensive. It may be argued that there are no strictly ostensive words at all. But, as philosophical arguments go, it does not seem notably silly. As for the identification of actual and correct use, Mr Gellner is cheerfully confident that its hash can be adequately settled by a reference to the naturalistic fallacy. But ' correct use ' here does not mean ' ideal use ' or anything like that; it means ' use that is correct in terms of the established and prevailing rules of language '. Where else are we to look for these rules but in the concrete detail of our linguistic practice? This may seem a trivial point, for have not linguistic philosophers gone on to say that actual, correct, established use is ideal, that it cannot be improved on? I think Mr Gellner is right to suppose that they have sometimes spoken as if they did believe this but in fact no such sweeping contention is required to justify their procedure or would be generally accepted by them. What they would say is that the first task of philosophy is to get a clear understanding of the conceptual system that we actually have before embarking on reforms. Wittgenstein's more severe view was that the acquisition of such an understanding was the *sole* purpose of philosophical inquiry. But neither view rules out linguistic innovation in general, they only contend that it is a secondary, consequential business for philosophers or, more rigidly, not their business at all. One does not become Canute by failing to assist the incoming tide. There is, anyway, a certain artificiality about Mr Gellner's defence of linguistic reform, for the notion of a philosopher as a conceptual innovator is itself a by-product of linguistic philosophy.

Mr Gellner nowhere does justice to the power and originality of what may be reasonably called the central insight of Wittgenstein's later thought (the crystallisation of a suspicion suggested by the work of G. E. Moore): the theory that the main deliverances of the epistemological tradition are marked by a characteristically paradoxical quality which is produced by, and can be explained by the recognition of, the distortion of the common meaning of words occurring in the arguments leading up to them, to which we are led by seductive models and analogies of their working. Now, right or wrong, this is a forceful and fascinating hypothesis. There is nothing evasive about it. It was presented in a clear and unequivocal manner by its inventor. There is a perfectly definite method of verification attached to it, namely the discovery of effective explanations of the paradox-producing distortions, of real cures for philosophical headaches. Even though many who have been much attracted by it would now admit that it has not been very convincingly verified, and that it indicates a contributing factor in philosophical perplexity rather than gives a total explanation of it, they would rightly hold that it has entirely and irreversibly changed the face of philosophy. For it has made it impossible henceforth to take the more counter-intuitive pronouncements of traditional philosophy at their face value as statements of some higher truth about the world than can be communicated by the propositions of science and commonsense. And nothing supports this more effectively than Mr Gellner's contented acceptance of the view that true philosophy is conceptual reform.

Wittgenstein's asceticism about innovation is no longer a matter of theory: for Austin it was a question of establishing a rational order of priorities, for Mr Strawson (in *The Revolution in Philosophy*) it is, as well as this, a matter of personal preference and interest though he is sceptical about philosophical innovation to the extent of seeing the construction of ideal languages as largely the provision of analytical tools for the greater understanding of our actual conceptual system in much the same way as a modern economist might see the relation between classical, Ricardian economic theory and the realities of economic life. An essential point to notice is that conceptual innovation by past philosophy has been generally unconscious and unintended. When Professor C. I. Lewis says 'no empirical statements are certain' he is presenting what he believes to be an important discovery, not a redefinition of 'certain' as 'completely verified'. It is to concede at least half the linguistic philosopher's claim about ordinary language to recognise as innovation what has hitherto been regarded as discovery.

All the same there is some justice in Mr Gellner's assault on the general attitude towards speculative innovation adopted by linguistic philosophers. The Austinian philosophy of present-day Oxford derives as much from the local school of Cook Wilson and Prichard as it does from Wittgenstein and it does tend to share some of that school's pedantic obsession with the conceptual *status quo*, an outlook which found its most farcial and exemplary expression in H. W. B. Joseph's attempted demolitions of Freud and Einstein. Again his strictures on Austin's evolutionary argument in favour of ordinary language have a good deal of force though he rather excitedly overrates the importance its propounder attached to it. Austin's point was that there was good reason for taking ordinary language seriously and for doubting whether it could be blithely written off as a tissue of inconsistencies and be reformed or reconstructed *a priori*. His argument is a caution against linguistic utopianism. And conservatives are not the only opponents of utopias. Nevertheless the general tendency of ordinary language philosophy has been towards an ultimately uncritical acceptance of the established conceptual order. Mr Gellner's attack on it comes at a time when it is already changing its direction on account of internal developments within linguistic philosophy, above all perhaps a realisation that philosophical problems obstinately refuse to give up the ghost however minute and delicate the methods of ordinary-language analysis that are applied to them. Another factor has been an increasing doubt about the solidity and definiteness of the boundary between the philosophical and the ordinary. Mr Gellner's criticism of the pretension to neutrality which is based on faith in this boundary correspond, in their somewhat boisterous way, to the doubts of many linguistic philosophers.

I shall not spend much time on Mr Gellner's attack on the fourth pillar of linguistic philosophy, the polymorphism that insists on the infinite and ineffable variety of language, and on the flight from theory that it supports since I agree with it. Here again, however, it is necessary to distinguish between Wittgenstein, for whom the impossibility of general philosophical theories was a matter of doctrine, and the Oxford philosophers for whom it is a counsel of prudence. As a doctrine it is plainly inconsistent, not only with Wittgenstein's actual practice, for all his bewildering attempts to stick to pure description, but also with itself. Its powerful hold over Wittgenstein means simply that the interpretation of his later works is made laborious in a new but not very interesting way. There are plenty of instances of the extraction of general theses from the later philosophy of Wittgenstein and the writings of such followers as Professor Malcolm reveal in practice an admission that some of them would be prepared to concede in principle: that the doctrine of philosophical ineffability is an aberration inherited from the *Tractatus*. Its source would seem to lie in the consideration that philosophy, understood as an inquiry into the relation between language and the world, can only put itself outside the language it is concerned with by saying nothing. But this is a *reductio ad absurdum* not a conclusion to rest content in. In order to discuss language you have to use it, a state of affairs in which it is necessary to be on your guard against begging the question. We are back in Neurath's boat, language cannot be investigated in a condition of perfect linguistic innocence. The best corrective for this conceptual variety of original sin is imagination, the envisaging of languages deeply and substantially different from our own. As a counsel of prudence the polymorphic avoidance of theory is unattractive and Mr Gellner's thundering indictments of evasiveness have some foundation. But evasiveness is neither a monopoly of linguistic philosophers nor is it generally characteristic of them. The vanity of thinkers will always look for a refuge from criticism, as much in the vague woolgathering of Bradley or the non-committal, tolerant hypothesising of the later Carnap as in the voluptuous revelling in multiplicity of linguistic philosophers. And on the other point, it is hard to think of a less adequate description than 'evasive' for Professor Ryle and Mr Strawson. *The Concept of Mind*, it might well be said, is not evasive enough. Moore, Wittgenstein, and Austin were all, in their different ways, unsystematic philosophers: Moore by a kind of temperamental accident, Wittgenstein as a matter of theory, Austin through a methodological decision. One can agree with Mr Gellner that this is a defect while thinking that it is immensely outweighed by their positive qualities when they are compared with, for example, Lotze or Hermann Cohen or Alexander.

In general, then, Mr Gellner fails to upset commonsense naturalism, partly through inability to identify it clearly enough, partly through misrepresenting it as an unsupported dogma; his arguments against the linguistic philosopher's concern with ordinary language are weak in detail, and implicitly concede a justification for this concern which is never openly admitted, but nevertheless they have a genuine bearing on an emotion of uncritical reverence which has tended to develop from the concern; and, finally, his attack on the flight from theory, both as a principle and a practice, though well-founded, subverts only a peripheral and dispensable aspect of linguistic philosophy. On the whole his examination has three main deficiencies: its violence, its distortion and, oddly enough, its evasiveness. The violence does not matter very much; except perhaps tactically in that his book will evoke only annoyance in the people to whom it is addressed, for it is exclusively negative in intention and execution (unless, of course, sinuous creatures that they are, they glide out of the line of fire and give themselves up to the pleasures of *Schadenfreude*). The distortion consists in his remorseless attention to philosophical methodology and his general neglect, except for brief, exemplary notice, of the concrete philosophical work of his victims. To take just one example, it is extraordinary that Mr Strawson's criticism of formal logic, one of the most solid achievements of recent Oxford philosophy, is not even mentioned. Mr Gellner's evasiveness lies in his resolute refusal to reveal his own philosophical opinions or to define what he regards as the proper method of philosophy in any but the vaguest imaginable terms. It is perhaps an Oxford sense of decorum that inhibits me from attempting a sociological, as well as a philosophical, *tu quoque*. Incidentally his sociology is empirically shaky: logical positivism was a philosophy for gentlemen, linguistic philosophy is a somewhat more solidly middle-class affair. It is also a much less monolithic business than Mr Gellner makes it out to be (this belief, like the diagram, half joke, half obsession, has a slightly paranoiac flavour). Only a minority of Oxford philosophers come anywhere near fitting his description and the intellectual tone of that university is not the sole responsibility of philosophers. The unremitting hostility of historians, for example (voiced in Professor Trevor-Roper's review of *Words and Things*) is a discordant element which conflicts with Mr Gellner's picture of undisturbed complacency. But still, linguistic philosophy has its faults and it is a good thing for them to be pointed out even in the exaggerated outlines of a caricature.

Anthony Quinton

The Study of Man. The Lindsay Memorial Lectures 1958. By Michael Polanyi.
Routledge & Kegan Paul, London, 1959. Pp. 102. 7s. 6d.*

Dear Dr Wisdom,

I struggled with this book recently and the more I think over it, the less I feel happy with some of the basic concepts Polanyi develops, whereas I still admire some others.

On the one hand he is saying that every understanding is tacit with human beings *and* animals, and on the other hand that every comprehension is an understanding, a grasping of disjointed parts into a comprehensive whole. Now I don't think that even Köhler's apes *understood* what they did although they certainly constructed a whole in order to obtain the banana.

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* Editorial footnote: What follows is a letter we received from Professor van Lennep explaining why he could not review the book. We considered that the letter itself constituted an interesting review and accordingly, after obtaining Professor van Lennep's permission, we are printing it as a review.

His equalisation of intellectual knowledge with knowing how to do something (skill) is, as far as I can see, playing with the ambiguity of the the word knowledge. The crux of all this probably lies in the first sentence of his book: 'Man's capacity to think is his most outstanding attribute'. I would say: his capacity to reflect (= self-awareness, selfconsciousness) is what distinguishes him from animals, because this is the ontological fact which is responsible for the possibility of using language, i.e. symbols of all kinds. Therefore although there is no understanding without comprehension, there is comprehension without understanding.

But Polanyi says (p. 20) the word which covers them all (i.e. all comprehending experience) is simply understanding.

Skills in the sense of Polanyi are tacit knowledge and it is true that skills are generally not explicit, but is it not just playing with words to speak of *tacit knowledge*? Is knowledge not understanding in a reflective way what we comprehend tacitly?

Polanyi says no, because the act of understanding what science has made explicit remains a tacit act that in itself cannot be made explicit.

Understanding, however, I would say is a comprehension of things that are disconnected from their biological importance to the subject, and this is reserved to the human being. Although we say that a bird 'knows' how to build a nest he certainly does not *understand* his own procedure. And I just don't understand what Polanyi means when saying that understanding is a valid *form* of knowledge. What other forms are there?

Now I consider Polanyi a great man, and I very much admired his book *Personal Knowledge*, but as far as I am concerned I would consider this booklet less clear and convincing than the first one.

And since I apparently do not understand clearly his theory of understanding, nor appreciate his conceptual framework in the first chapter (the other two chapters are much clearer and I am more agreed with them) I feel that I am not sufficiently at one with him to criticise him in a review and not competent enough. A true epistemologist and not a simple applied psychologist should be much more up to the standard to do this job, the more so since he considers his own theory a *theory of knowledge*.

Therefore I am afraid that I must abandon my original plan to review his *Study of Man*, with my most sincere apologies for my late decision.

D. J. van Lennep

Social Theory and Social Structure. By Robert K. Merton.
Revised and enlarged edition. The Free Press, Glencoe, Illinois, 1957.
Pp. 645. $7.50.

Robert K. Merton is not merely a professor of Sociology, he is a symbol and a programme. The volume under review is an expanded version of a work which has already made its mark and probably had a formative influence on many of those working within American sociology. What is he a symbol of? Not a simple empiricism, though empiricism is an ingredient. The empiricism which is associated with his programme is not so much an anti-transcendentalism, as past empiricisms have been; but a kind of high-minded puritanism: a deliberate determination in the interests of a far off salvation not to wallow now in the pleasures of interesting thought. Thought is not excluded, by any means: but it is a severely self-conscious methodical thought, preoccupied with its own propriety. Here perhaps we find the Calvinist personality of sociological thought, willing (or anxious?) to postpone present cognitive gratification in the interests of ultimate salvation.

What lies at the root of this? The acute awareness of sociology as a kind of under-developed country amongst the sciences, and a determination to set this right and catch up. As in these poor countries, the long-term policies required to escape under-development may conflict with the need to do as much good as possible at once.

There is a well known schema for what under-developed countries need: an adequate infra-structure, and a class with creative and entrepreneurial spirit. Professor Merton tries to supply, or aid the supply, of both these requirements. In other words, the book contains attempts both at the furthering of social theory and actual instances of handling of sociological problems. His work on the clarification of the much favoured 'functionalist' approach is already well known. On the substantive side, philosophers of science will be particularly interested in his studies of the social background of science.

Whether this self-conscious, long-term, and deliberate approach really will bear fruit, I suppose only time can tell. Professor Merton is superior to some he would call his masters. His followers may easily be led into stilted mumbo-jumbo. But with his preoccupation that sociological work should become cumulative one can strongly sympathise. And what are the alternatives? A pure and rigorous empiricism in sociology misses a good deal. At the other extreme, *weltgeschichtliche Betrachtungen*, though by no means to be despised, do not easily lend themselves to fruitful and diffused teaching. Even if they did, what would one do with a really large output of them?

So, in the meantime, it is possible that Professor Merton's choice is a right one, if perhaps not the uniquely right one. It is a pity that the path of virtue should sometimes seem such a stony one.

ERNEST GELLNER

Science and the Creative Spirit. Ed. by Harcourt Brown.
University of Toronto Press, 1958. Pp. xxvii + 165

FIVE representatives of various fields of humanities have produced this set of essays destined to throw several distinct beams of light upon their common theme, viz. an evaluation of the influence exerted by humanities and sciences upon each other. What gives the volume its particular interest is the attempt to examine this problem by a team of humanists taking deliberately the attitude of 'critical sympathy' for science with the view of promoting a better understanding between the two camps.

The introductory article by Harcourt Brown, Professor of French at the Brown University, deals with basic concepts and definitions. Humanities are defined as being traditionally associated with 'the study of the record mankind has set down in books'; they are emphatically contemplative whereas sciences are preponderantly analytical; their primary subject is 'the creator and the creative imagination of man', 'the domain of the muses'. The sciences might be admitted as a part of this domain, if taken in their totality and without distinction between basic and applied sciences. The objective impersonality of science is in contrast with the 'self-communicative' ways of the humanist. True achievements of art can stand alone and independent, while those of the sciences never can be more than parts of something, like 'bricks in a cathedral'. In spite of their influential position, the sciences so far have failed to 'persuade' the modern humanist; for in the last instance science tends to destroy human values, being essentially a 'calculation of forces'. Similar ideas of Bertrand Russell of 1920 are quoted in conclusion.

Karl W. Deutsch, Professor of Political Science at the Massachussetts Institute of Technology, treats the subject of the scientific and humanistic knowledge in the growth of civilisation. Preservation of integrity and wholeness in the artistic interpretation of reality is contrasted by the isolating methods of sciences; and the singularity of situations dealt with by man in the arts, by the 'repetitiveness' of those studied by science. J. B. Connant's idea of science being accumulative in contrast to the art, is accepted with the reservation that the latter, too, is accumulative in a sense. Similarities in the scientific and artistic minds and in their respective ways are stressed, and their interplay in the growth of our civilisation is discussed and illustrated.

F. E. Priestley, Professor of English at the University of Toronto, deals with the effect of scientific progress upon the imagination in English literature. 'Significant ordering of experience' is a fundamental feature common to the arts and sciences, in spite of the differences in the working methods. Scientific influences in English literature are followed down to the beginnings of the Royal Society, with Jonathan Swift as the representative critic of the sciences of that epoch. The periods that follow are equally characterised by an interesting sequence of names, up to Charles Darwin's outstanding impact.

Harcourt Brown's article on the effect of sciences upon the development of French literature represents Blaise Pascal as the main figure characterising the initial period. Important English influences appear to have acted upon the relationship between sciences and arts in France; but the course the process took there was much different from the cognate process in England.

The concluding essay is written by a philosopher, Professor David Hawkins of the University of Colorado. The modern humanists have failed so far to understand the spirit of science, although every field of human creativity, especially that of philosophy, has been strongly influenced by science since the Renaissance. Connant's distinctive criteria of the artistic and scientific thinking cannot be accepted in their sharp formulation. Science is recognised as suitable to be placed 'alongside, if not among, the humanities'. A refreshing flash-history of the development of the scientific spirit and its influence illustrates these arguments, again with particular stress laid on the integrative and disintegrative ways of thought.

The papers have been trimmed so as to avoid major discrepancies in the individual outlooks, but they still have preserved strongly personal characters. The challenge of the book is so much the greater. The gaps are unavoidable in any work of this kind, and are obvious, being another added challenge. A scientist will be stimulated to much thought and comment on every page, and will close the book with a regret that the limitation of the study to the English and French area did not allow discussing such outstanding figures as Leonardo de Vinci and Goethe, both highly relevant for the general theme. Goethe's *Faust* contains many an invective against the Cartesian philosophy; Goethe's scientific contributions still are valued; and his interest in Purkynje and other 'heautognostic' physiologists might be considered as physiognomical for the subject. Another topic which would interest the scientist and which is only indirectly touched in the volume, is the influence of sciences upon art apart from literature, e.g. painting, sculpture, architecture, and music.

If this sample of opinions is representative of what the humanists nowadays feel about science, the impression is that of a defensive, or at least embarrassed attitude. Surprisingly little is said by the authors about the general moral problems connected with the growth of science. The voice of artists and humanists in this matter of undeniable urgency should be heard as a strong second fiddle to the first of the contemporary science.

JAN BELEHRADEK

Evidence and Inference. Edited by D. Lerner.
Free Press, Glencoe, Illinois, 1959. Pp. 164. $4.00.

A SYMPOSIUM of such generality as this makes a very tantalising book. There are many extremely interesting suggestions in the various contributions, but the symposiasts have had to leave them undeveloped. Three contributions are worth singling out for particular mention; Raymond Aaron's remarks on the nature of historical and sociological ' individuals ' (in the logical sense); Henry Hart and McNaughton's discussion of the law, particularly in view of some recent attempts to draw parallels between inductive inferences and legal verdicts; Erikson's account of the clinical method in psychiatry.

Aaron argues that while for historians individual human beings are the atoms, they are not always the units of the discourse. Units depend upon the interaction of several factors. For instance a battle is a unity for some historical purposes, because it is *both* spatio-temporally located and organised by the interaction of the plans of the commanders. However when we come to such historical units as ' periods ' and ' cultures ' we have no such clearly objectifiable individuating devices. What is chosen as the individual is more and more arbitrary the more diffuse it becomes. The formation of such individuals is a useful schematism but ' the error originates when, departing from the facts which prove the diversity and equivocal reality of *ensembles*, one infers a kind of metaphysics which transforms these *ensembles* into living beings, fated to be born and to die '.

In discussing the operation of the law Hart and McNaughton emphasise the peculiarity of the legal treatment of fact. The ' most distinctive practices of the law ' they take to be found in the ' formal and official settlement of a controversy '. That is, as they put it, when a case reaches the courts, ' victory, and not accommodation is the object of the parties '. So that truth is what is left over from a process of legal attrition, rather than the total set of propositions established beyond reasonable doubt.

The discussion of clinical psychology also emphasises the difference between this kind of activity and science proper. Here, the mutual interaction of patient and therapist is the essence of the curative process. Nothing in the way of general law is invoked in what turns out to be a unitary process of diagnosis and treatment.

There are also contributions on physics (M. Deutsch), sociology (P. Lazarfield), and physiology (J. Fine). Each of these contains interesting, suggestive but sketchy material. There is an introduction by D. Lerner, designed to show the contrast between totalitarian and empirical theories of science.

R. HARRÉ

RECENT PUBLICATIONS ON THE PHILOSOPHY OF SCIENCE

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ERRATA

In the article-review 'Wittgenstein's Theory and Practice of Philosophy' by G. Kreisel, this *Journal*, 1960, **11**, the following two corrections should be made:

page 242, note 3 for 'Kleene and Mostowski' read 'G. Kreisel'.
page 251, note 1 in the last line, for 'note 2' read 'note 3'.

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